The Labor Market Effects of Expanding Overtime Coverage

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Abstract

This paper studies the employment and income effects of a federal proposal in 2016 to expand overtime coverage to additionally cover salaried workers earning between \$455 and \$913 per week (\$23,660 and \$47,476 per annum). Although the policy was unexpectedly nullified a week before its proposed effective date, using detailed administrative payroll data covering one-sixth of the U.S. workforce, I find clear evidence that firms nevertheless responded to the policy by bunching workers' earnings at the new \$913 exemption threshold. On average, the base salary of directly affected workers who remain employed increased by 1.4%. Meanwhile, for every hundred workers who would have gained coverage under the policy, 10 jobs were reclassified from salaried to hourly. Preliminary analysis also suggests that there may have been negative employment effects. Examining the distribution of these margins of adjustments, I find that the positive income effect accrued entirely to workers who were bunched at the \$913 threshold but would otherwise have earned between \$720 and \$913 per week, whereas the reclassification and negative employment effects were spread across jobs paying within the entire range of newly covered base salaries.

JEL codes: J23, J31, J33, J38

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1 Introduction

A key factor in the determination of employment, wages, and hours in the economy is the extent to which labor market regulations affect labor demand. Interest in this topic has sparked a large literature on the labor market impacts of the minimum wage, its implications for labor market efficiency, and its role in the rise of income inequality (e.g. see Brown, 1999). In contrast, although overtime regulations could potentially impose similarly large distortions on firms' employment decisions, far less is known about its effects on the labor market, especially in the United States. The original motivation for imposing an overtime premium was the theory that it would encourage firms to spread hours across workers, thereby reducing the prevalence of long workweeks and increasing employment (Ehrenberg, 1971). However, a competing economic theory argues that in equilibrium, overtime coverage has no real labor market effects if straight-time wages can lower such that workers earn the same amount before and after coverage (Trejo, 1991). Underlying these models are core assumptions about the flexibility of wages and hours. Despite the long history of overtime regulation in the US, there is still no consensus on the effects of overtime eligibility on the labor market.

Empirical studies of overtime in the United States have been limited by a lack of clean policy variation and imprecise data.[1] Previous studies, exploiting variation from the expansion of overtime coverage to additional industries and demographic groups, have found a mix of negative (Costa, 2000; Hamermesh and Trejo, 2000) and no significant effects (Johnson, 2003; Trejo, 2003) of overtime coverage on hours. However, since these expansions in coverage often coincided with changes in the minimum wage, tests of the income effect of overtime have instead relied on cross-sectional variation in eligibility to estimate the correlation between log-wages and overtime hours, by eligibility status (Trejo, 1991; Barkume, 2010).[2] While the negative relationship identified in these studies are consistent with firms lowering wages to partially negate the costs of overtime requirements, they can also be driven by the selection of low skilled workers into jobs that demand long hours. In addition to the shortage of clean policy variation, existing studies have also been limited by the level of aggregation in household surveys. Without measures of employment at a firm or establishment level, it

[1] See Hart (2004) and Brown and Hamermesh (2019) for an overview of the literature on overtime.

[2] An exception is Johnson (2003), which studies both the hours and wage effects of a Supreme Court ruling that extended coverage to public state and local employees. However, the direction of his estimated wage effects vary depending on the specification.

is unclear whether one can precisely estimate changes in aggregate employment, even with an ideal natural experiment. Given these empirical challenges, "no study presents estimates of effects [of overtime coverage] on employment, and none offers evidence on all outcomes: [wages, earnings, and hours]" (Brown and Hamermesh, 2019).

My paper fills this gap in the literature by exploiting recent federal and state expansions of overtime coverage for low-income salaried workers. Labor regulations in the U.S. stipulate that salaried workers are legally covered for overtime if they earn below an "overtime exemption threshold" set by the Department of Labor (DOL). While this threshold varies across states and over time, the current draft of my paper will study the effects of a specific attempt to increase the federal threshold.[3] In May 2016, the Department of Labor announced that they would more than double the federal overtime exemption threshold from \$455 per week (\$23,660 per year) to \$913 per week (\$47,476 per year). The new rule was scheduled to take effect starting December 1, 2016 and would have guaranteed overtime coverage to over a third of all salaried workers. However, one week before the effective date of the new rule, a federal judge placed an injunction on the policy. Although the new threshold never went into effect, firms were aware of the the rule change in the period between May 2016 and December 2016, and so had opportunity to respond in anticipation of the new rule.

To evaluate employers' response to the expectated expansion in overtime eligibility, I analyze the evolution of the firms' income distribution following the announcement of the policy using detailed anonymous monthly administrative payroll data structured at the employee-employer level. Comparing the frequency distribution of salaried workers' weekly earnings between April and December 2016, I find clear evidence that employers attempted to keep workers exempt from overtime by bunching workers' salaries at the proposed \$913 threshold. To identify the causal effect of the policy, I model the counterfactual change in the frequency distribution in 2016 using a linear transformation of the difference between the April and December 2015 distributions. This enables me to compute the employment effect of the policy from the difference between the observed and counterfactual difference in distributions.[4] In my analysis, I estimate the employment effects separately for salaried and hourly jobs. Furthermore, I also apply the difference-in-distributions strategy to compute the income effect of the policy by weighting the frequency distributions by workers' weekly pay.

I find that raising the overtime exemption threshold reduced aggregate employment but

[3]I will present the results of the state-year analyses in a subsequent revision of this paper.

[4]See Cengiz *et al.* (2019) for a recent analysis of how the minimum wage impacts employment at different parts of the wage distribution

increased average salaries. My current analysis estimates that employment fell by 0.069 (s.e. 0.036) jobs for each salaried worker initially earning between the old and new thresholds. However, this estimate is sensitive to various sample restrictions that will be explored in more detail in upcoming revisions of this paper. As such, I am unable to make any conclusive statements about the effect of the policy on total employment, but my current specification would suggest that there are negative employment responses.

The negative employment response to the policy is inconsistent with the theory that overtime legislation stimulates job creation by encouraging firms to reduce long hours and increase employment (Ehrenberg, 1971). Previous tests of this hypothesis have generally found imprecise negative employment effects of policies outside the US that reduced the standard number of hours in a workweek (Hunt, 1999; Crépon and Kramarz, 2002; Skuterud, 2007; Chemin and Wasmer, 2009). However, unlike an expansion in overtime coverage, shortening the workweek only raises the marginal cost per hour of those working no more than the initial standard weekly hours. For individuals already working overtime, a shorter workweek only increases their average cost. Thus, the theory predicts that expanding overtime coverage would have a greater positive effect on employment than shortening the workweek. Nevertheless, I find that the proposed overtime expansion in 2016 yielded negative employment effects, reinforcing existing evidence that work-sharing policies, implemented through the regulation of overtime eligibility, are ineffective tools for generating employment.

While the jobs displaced by the policy were paying between \$455 and \$913 per week, the weekly wage bill of the average firm only decreased by \$73.19 per job lost, implying that average incomes must have increased. Restricting the sample to only job-stayers, I estimate that the base pay of the average affected worker increased by 1.4% (s.e. 0.2%). In contrast to the negative employment effects, which were spread across all newly covered jobs, only workers who would have otherwise earned between \$720 and \$913 per week saw an increase in their weekly earnings. I find no evidence that firms reduce workers' base pays in response to having to pay overtime, thereby rejecting the prediction of the "contract model" that firms reduce workers' base salaries to compensate the firm for the additional costs of overtime pay (Trejo, 1991). To rationalize the effects of the policy, I develop a job-search and matching model wherein firms and workers bargain over jobs' the weekly base income, weekly hours and pay classification (i.e. salaried/hourly status). I show that my results are consistent with the existence of either frictions from wage rigidity or the cost of monitoring workers' hours, and that without these frictions, overtime coverage would simply cause workers and firms to cut base pay as predicted by the contract model.

Consistent with the existence of labor market frictions, I find that firms also restructured their production to utilize more hourly workers and fewer salaried workers. Splitting my sample by salaried and hourly status, I estimate that for each worker directly affected by the new threshold, 0.100 (s.e. 0.006) workers are reclassified from salaried to hourly. Reclassification explains a larger percentage of the decline in the number of salaried workers earning between the old and new thresholds than either the employment or bunching effects. I find that the bunching effect primarily benefits workers who remain salaried after the policy, whereas reclassified workers receive a wage approximately equal to a fortieth of their base pay prior to reclassification. The reclassification effect suggests that many of the properties often associated with salaried jobs relative to hourly jobs, such as higher pay, longer hours, and more flexible work schedules (Mas and Pallais, 2020), may not simply be an intrinsic feature of jobs' duties, but also a result of firms' response to the overtime exemption policy to classify low-income covered employees as hourly.

The results of my study inform the debate surrounding the many federal and state policies to increase the overtime exemption threshold. In addition to a recent federal policy that raised the FLSA threshold to $684 per week on January 1, 2020, many states have also begun imposing their own thresholds that exceed the federal one.[5] Limited by the dearth in research on this topic, the federal Department of Labor's cost-benefit analysis of its policy relies heavily on strong assumptions that incorporate a combination of the predictions from both theories of overtime, and elasticities from the literature.[6] My results reject the predictions of previous models of overtime, and by extension, many of the conclusions of the DOL's analysis. Furthermore, I am able to estimate employment and reclassification effects, which cannot be extrapolated from elasticities in the literature, even with strong assumptions.

The remainder of the paper is organized as follows. In section 2, I explain the institutional details governing U.S. overtime regulations and the specific policy to expand coverage for salaried workers. Section 3 outlines the predictions of the two competing models of overtime and nests them within a general labor demand framework. In section 4, I describe the admin-

[5]Over the last decade, California and New York incrementally increased their thresholds to $1040 and $1125 per week, respectively. Effective January 1, 2021, Washington and Colorado will raise their thresholds to $965 and $778.85 per week, respectively. Michigan and Pennsylvania are in the process of writing proposals for raising their salary thresholds above the federal one.

[6]For instance, the DOL assumes that of the workers who occasionally work overtime, half would have both their base salary and hours decreased, and half would receive overtime compensation with no change in base pay or hours. 84 FR 10900

istrative payroll data from ADP LLC that I use in this study. In section 5, I present graphical evidence that the nullified policy had a binding effect. Section 6 outlines my identification strategy and reports my results on the aggregate employment and income effects. In section 7, I decompose the specific margins by which firms adjust their labor force. I conclude in section 8 by discussing the implications of my findings and areas for future research.

2 Federal and State Overtime Regulation

The Fair Labor Standards Act (FLSA) requires employers to record workers' hours, and pay them one and a half times their regular rate of pay for each hour worked above 40 in a week.[7] While this rule applies to nearly all hourly workers in the US, the FLSA exempts a large group of salaried workers from overtime coverage who are considered executive, administrative, or professional (EAP) employees. To exempt a salaried employee as an EAP, the employer must show that the worker 1) primarily performs white-collared tasks as defined by the Department of Labor (duties test), and 2) earns a weekly salary equal to or greater than an "exemption threshold" set by the DOL (salary levels test).[8] Conversely, salaried workers earning below the exemption threshold, as well as blue-collared salaried workers earning above the threshold, are covered for overtime pay.

In addition to the federal regulation, multiple states also impose their own overtime exemption thresholds that exceed the one set by the FLSA. In particular, California, New York, Maine, and Alaska each define their overtime exemption thresholds as a multiple of their respective minimum wages. Thus, each time these states raise their minimum wage, their overtime exemption threshold simultaneously increases according to a known formula. In all four states, the overtime exemption threshold is high enough such that the segment of the income distribution affected by changes in the threshold does not overlap with that of the minimum wage, even after accounting for potential spillovers.[9]

[7]For hourly workers, the regular rate of pay is simply their wage. For salaried workers, the regular rate of pay is defined as their weekly salary divided by the number of hours for which the salary is intended to compensate (29 C.F.R. § 778.113). In practice, and as verified using payroll data, firms typically calculate salaried workers' regular pay rate as their weekly salary divided by 40. For example, a worker paid a salary of \$450 per week has an implied wage of $\$11.25 = \frac{450}{40}$. If the worker is covered for overtime, she would receive $\$16.88 = 1.5 \cdot 11.25$ for each hour above 40 that she works in a given week, in addition to her regular salary of \$450.

[8]The law also makes exceptions for special occupations such as teachers and outside sale employees. For a detailed overview of the requirements to qualify as an EAP worker or other exemptions, refer to Face Sheet #17A published by the Department of Labor.

[9]The smallest scalar factor used to determine an overtime exemption threshold is in Maine, where

To identify the effects of expanding overtime coverage, I examine two sources of policy variation: 1) a rule change in 2016 that would have doubled the federal threshold but was unexpectedly nullified, and 2) smaller federal and state-level increases in the overtime exemption threshold. These increases extend overtime eligibility to workers with salaries between the old and new thresholds who were previously exempt from overtime. I present in appendix figure 1 all state and federal overtime exemption thresholds since 2005, along with the invalidated proposal in 2016. While I describe in this section both sources of policy variation in overtime coverage, my current draft of the paper will present only my analysis of the large 2016 FLSA policy. The results of my event-study analysis will be included in the subsequent revision of this paper.

At the federal level, the percent of salaried workers guaranteed overtime compensation under the FLSA fell from over 50% in 1975 to less than 10% in 2016 (see Figure A.1). In an attempt to restore overtime coverage to salaried workers, the Department of Labor announced on May 18, 2016 that it would increase the federal exemption threshold from \$455 per week (\$23,660 per year) to \$913 per week (\$47,476 per year) starting December 1, 2016. According to the Current Population Survey, the new rule would effectively raise the threshold from the 10th percentile of the salary distribution to the 35th percentile. However, to employers' surprise, a federal judge imposed an injunction on the policy on November 22, 2016, stating that such a large increase in the threshold oversteps the power of the DOL and requires Congress approval. Given that this unexpected injunction occurred only one week before the policy was to come into effect, many companies at the time reported that they had either already responded to the policy and would not retract those changes, or made promises to their employees that they intended to keep.[10] After confirming that firms did indeed respond to the nullified policy, I use the 2016 proposal as a natural experiment to estimate the short-run effects of a large federal expansion in overtime coverage for salaried workers.

To complement my evaluation of the 2016 FLSA rule change, I implement an event

the threshold is equal to $\frac{3000}{52} \approx 58$ times the minimum wage. Assuming a standard workweek of 40 hours, this implies that salaried workers paid at the threshold earn 44% more than the minimum wage, well above the range that the literature has found spillover effects from the minimum wage (Brochu *et al.*, 2015; Cengiz *et al.*, 2019).

[10]For example, WalMart and Kroger raised their managers' salaries above the \$913 threshold and did not take back those raises after the injunction. See *Some Employers Stick With Raises Despite Uncertainty on Overtime Rule* - Wall Street Journal Dec 20, 2016. For a detailed recounting of firms' expectations leading up to and following the injunction, refer to appendix section B.

study analysis using 18 prominent federal and state-level increases in the overtime exemption threshold between 2014 and 2020.[11] Following the injunction of the 2016 rule change, the federal Department of Labor announced on September 13, 2019 that it would raise the FLSA overtime exemption threshold to $684 per week, effective January 1, 2020. Unlike the 2016 policy, the recent increase in the federal overtime exemption threshold did not affect all states. Since California and New York already had thresholds that far exceeded the new FLSA requirement, I use these unaffected states as control groups to model the counterfactual labor market outcomes in the absence of the new rule. Similarly, for each of the state-specific threshold increases, I am able to use the states that are bound by the FLSA threshold as a control group.[12]

In addition to the state-year variation in the overtime exemption thresholds, the nature of the overtime regulation also provides multiple other sources of variation that can be used as controls and placebos. These additional sources of variation are particularly important for identifying the effects of the federal policy in 2016, which affected all states at the same time. First, each rule change only directly affects salaried workers earning between the old and new thresholds, and therefore has little effect on workers with incomes much higher in the salary distribution. Second, the cost of a threshold increase differs by firms depending on the share of their workforce initially between the old and new thresholds. In particular, firms with no salaried workers in that range are unaffected by the policy aside from general equilibrium forces, and can therefore serve as a control. Third, given that some workers paid between the old and new thresholds were already eligible for overtime pay, I have worker-level variation by employees' initial exemption status. Forth, and specific to the 2016 FLSA regulation, since there were no changes to the federal overtime exemption threshold prior to 2016, I use earlier years as a placebo test to validate my empirical strategy.

While salaried workers who never work above 40 hours per week could arguably also act as a control, in my analysis, I consider these workers as part of the treatment group. From a labor demand perspective, one of the main concerns that businesses raised to the DOL

[11] I exclude the four most recent rule changes in Alaska that cumulatively increased the exemption threshold by only $35 to adjust for inflation. I also exclude the January 2014 event in New York due to coding issues with the data.

[12] Starting in 2017, California and New York passed legislation that generated variation within-state. California sets a lower threshold for employers with fewer than 26 employees, whereas New York's threshold varies by both employer size and location (i.e. in/near/away from NYC). Since the data I use only records the state of workers' residence and consists of firms with at least 50 workers, I do not make use of the within-state variation. When a state has multiple thresholds, I use the largest of its thresholds for my event study analysis.

in response to the 2016 rule change is the cost of monitoring salaried workers' hours. Thus, covering employees who never engage in overtime work nevertheless raises their cost to the firm. Furthermore, from a labor supply perspective, workers who engage in no more than 40 hours of labor per week may want to increase their hours once they are covered for overtime. As a practical matter, I also do not observe the hours of salaried workers in the data if they are not covered for overtime.

3 Theoretical Predictions

To guide my empirical analysis, I examine multiple theories of how overtime coverage may affect the labor market. The literature has developed two competing theories of overtime. The standard labor demand model argues that if overtime has no effect on wages, then it would raise the marginal cost per hour of labor for hours above 40 in a week, thereby incentivizing firms to substitute away from long hours for more employment (Ehrenberg, 1971). While the assumption that wages are fixed is highly restrictive, previous attempts to endogenize wages by integrating labor supply responses have generated intractable predictions (Hart, 2004). To model overtime in a market equilibrium, a competing theory argues within a compensating differentials framework that base wages would decrease in response to overtime coverage, such that total income remains unchanged (Trejo, 1991).[13] Under this framework, overtime coverage would have no effect on real income, hours, or employment. While this model allows for movement in both wages and hours, it assumes a frictionless environment that may not be true empirically. Furthermore, both theories of overtime were developed to model the effect of overtime for hourly workers. They do not make a distinction between salaried and hourly workers, nor allow for overtime coverage to depend on an exemption threshold.

In this section, I present a search and matching model that captures these institutional details, and generates a rich set of testable predictions of the labor market impacts of covering low-income salaried workers for overtime. In the first subsection, I describe the process by which workers' income, hours, and salaried/hourly status are determined in my model, assuming exogenous contract rates and a stationary environment. Next, I examine how these outcomes respond to the introduction of overtime coverage for salaried workers earning below

[13] For instance, suppose an employee initially works 50 hours for a salary of \$800 each week and receives no overtime. If this worker becomes covered for overtime, the firm can reduce the worker's base salary to \$581.82, so that with the 10 hours of overtime, the worker would continue to receive $\$581.82 \cdot (1 + 1.5\frac{50-40}{40}) = \$800$ per week.

a given threshold, and how these effects differ given fixed costs of monitoring hours or wage rigidity. Following the comparative statics analysis, I study the dynamic responses by endogenizing firms' vacancy creation decision via a matching function formulation (see Pissarides (2000) for a review of this approach). For conciseness, I focus on the intuition of the model, and defer formal derivations and proofs to appendix C.

3.1 Search and Bargaining with Exogenous Contract Rates

The basic structure of my model builds on the theory of minimum wage developed by Flinn (2006). Suppose unemployed workers continuously search for a job and match with potential employers at an instantaneous rate $\lambda \geq 0$. Each match is characterized by three parameters. As conventional, I assume each worker-firm match has a idiosyncratic productivity level, θ. To generate variation in hours and pay classification between jobs, I introduce two non-standard parameters: a disutility of labor that varies between workers $a \sim H(a)$, and a relative value of classifying the job as salary rather than hourly F. The match quality and salary-fit of jobs follow a joint distribution $G(\theta, F)$.[14]

When an individual and firm meet, they both observe (θ, F, a) and Nash-bargain over the weekly income (w), weekly hours (h), and pay classification (S) of the job. If the applicant's value of accepting the job, denoted by $V_e(w, h)$, exceeds the value of continued searching V_n, then the employment relationship is formed. While employed, I assume that workers do not engage in on-the-job search. If unemployed, the individual continues searching while receiving an instantaneous utility b. I assume jobs are exogenously destroyed at a rate $\delta \geq 0$. The instantaneous discount rate is $r > 0$. Given these parameters, I characterize the worker's value of employment and continued search by the following Bellman equations:

$$(r+\delta)V_e(w,h) = w - a^{-\frac{1}{\epsilon}} \frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + \delta V_n$$

$$rV_n = b + \lambda \int_{V_e(\theta,F) \geq V_n} [V_e(w(\theta,F), h(\theta,F)) - V_n] dG(\theta,F)$$

where ϵ is the worker's constant labor supply elasticity. Unlike common search and matching models, I assume that workers receive a disutility from working longer hours that is additively

[14] One can think of F as the difference between the benefits (e.g. more flexibility, no need to monitor hours, etc.) of paying a worker by salary and the costs (e.g. less incentive to work long hours, etc.). A distribution of salary-fit can be motivated by an agency problem where a worker's effort depends on the pay classification chosen by the firm, and the amount of information about effort and output per hour varies by occupation (Fama, 1991).

separable from their income.[15]

I model firms' production technology as a function of both the match quality and the hours of labor per week, $y = \theta h^{\beta}$. Given the parameters (θ, F) and wage contract (w, h, S), the firm's discounted stream of profits is denoted by

$$J = \frac{\theta h^{\beta} - w + F \cdot \mathrm{sgn}(S)}{r + \delta}$$

where $S = 1$ if the position is salaried, and $S = -1$ if hourly, and $\mathrm{sgn}(\cdot)$ equals the sign of its argument. The firm's production function assumes that the output of each employee is independent of the output of other employees. This modeling assumption thereby eliminates the ability of the firm to substitute between hours per worker and number of workers, which is a necessary feature of the standard labor demand model of overtime to generate positive employment effects.[16]

Given (θ, F, a), the Nash-bargained job characteristics is given by

$$(w, h, S) = \arg\max_{(w,h,S)} [V_e(w, h) - V_n]^{\alpha} \left[\frac{\theta h^{\beta} - w + F \cdot \mathrm{sgn}(S)}{r + \delta}\right]^{1-\alpha}$$

where $\alpha \in (0, 1)$ represents the worker's bargaining power. This problem has a unique closed-form solution, which I henceforth denote by (w_0, h_0, S_0). The values $h_0 = \left(a^{\frac{1}{\epsilon}} \beta \theta\right)^{\frac{1}{1+\frac{1}{\epsilon}-\beta}}$ and $S_0 = \arg\max_S \{F \cdot \mathrm{sgn}(S)\}$ both maximize the total match surplus. The weekly hours equates the marginal product per hour of labor with the marginal disutility per hour, $\frac{\partial J}{\partial h} = \frac{\partial V_e(w,h)}{\partial h}$. Since the pay classification only enters the firm's production function, a job is salaried if and only if F is positive. Given h_0 and S_0, weekly income is set as a weighted average of the worker's surplus and the firm's surplus, similar to standard applications of the search and matching model:

$$w_0 = \alpha\Big(\theta h_0^{\beta} + F \cdot \mathrm{sgn}(S_0)\Big) + (1 - \alpha)\Big(a^{-\frac{1}{\epsilon}} \frac{h_0^{1+\frac{1}{\epsilon}}}{1 + \frac{1}{\epsilon}} + rV_n\Big)$$

Heterogeneity in (θ, a) generate a joint distribution of weekly income and hours, whereas the distribution of F generates the share of salaried and hourly jobs.

Since workers only accept jobs where $V_e(w, h) \geq V_n$, not all matches will result in employ-

[15] The predictions of the model are invariant to including a additive preference for pay classification.

[16] One way of potentially generating this substitution is by introducing diminishing returns to firm-size via the firm-worker bargaining problem developed by Stole and Zwiebel (1996). Previous job-search models with firm-size have not included an hours (eg. Acemoglu and Hawkins, 2014)

ment. For each worker type a and salary-fit F, there exists a critical value $\theta_0^*(a, F)$ such that $V_e(w(\theta_0^*, F), h(\theta_0^*, F)) = V_n$ and the worker accepts the job if and only if $\theta \geq \theta_0^*$. Inputting θ^* into the worker's value of unemployment, I derive V_n as a function of model primitives:

$$rV_n = b + \lambda \int_{\theta \geq \theta_0^*(a,F)} \left[\frac{w_0(\theta, F, V_n) - a^{-\frac{1}{\epsilon}} \frac{h_0(\theta, V_n)^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n}{r + \delta} \right] dG(\theta, F)$$

3.2 Comparative Statics in Response to Overtime Policy

Equipped with the benchmark model, I explore how the job characteristics (w, h, S) and the share of matches that become employment contracts change once I introduce an overtime premium. Henceforth, I make a distinction between base pay and gross pay. Let w present workers' weekly base pay, prior to receiving overtime compensation. The worker and firm bargain over the weekly base pay, weekly hours, and pay classification. However, the worker's value of employment and the firm's profit depend on gross pay $g = (1 + \eta_{(w,h,S)})w$, where

$$\eta_{(w,h,S)} = \begin{cases} \frac{0.5(h-40)}{40} & \text{if } h > 40 \text{ and } S = -1 \\ \frac{1.5(h-40)}{40} & \text{if } h > 40,\ S = 1, \text{ and } w < \bar{w} \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

and $\bar{w}$ represents the overtime exemption threshold.

First, I consider the case with no monitoring costs or wage rigidities. Since neither the firm's production technology nor the worker's preferences change, their agreed upon job characteristics is equivalent to the benchmark case without an overtime policy. Workers' base incomes are discounted by a factor of $(1 + \eta_{(w,h,S)})$ relative to w_0 so that their gross incomes remains the same: $g = w_0$. Weekly hours, pay classification, and reservation match quality are the same as the baseline case. This result is analogous to the predictions of the compensating differentials model of overtime where base income adjusts such that overtime coverage has no real labor market effects (Trejo, 1991).

Second, I examine the case where it is costly for the firm to monitor workers' hours. This cost corresponds to the FLSA's requirement that employers keep a record of the hours worked by all employees covered for overtime. Since firms already monitor the hours of hourly workers, this friction only raises the costs of salaried workers earning less than the overtime exemption threshold. The firm's discounted stream of profits is given by

$$J = \frac{\theta h^{\beta} - (1 + \eta_{(w,h,S)})w + F \cdot \mathrm{sgn}(S) - C \cdot 1[S = 1, w < \bar{w}]}{r + \delta}$$

where C is a constant, and $1[S = 1, w < \bar{w}]$ is an indicator that equals 1 if $S = 1$ and $w < \bar{w}$.[17] The monitoring cost does not affect the bargaining outcome of hourly jobs or salaried jobs that pay above the threshold in the baseline scenario. For newly covered jobs that if not for the overtime policy would be salaried $S_0 = 1$ and pay $w_0 < \bar{w}$, the monitoring cost has one of three possible effects on the Nash bargaining solution, depending on the match quality and salary-fit (θ, F):

Prediction 1 (Bunching): If the job's income in the benchmark scenario is sufficiently close to the overtime exemption threshold (i.e. $\bar{w} - w_0$ is small), then the Nash bargaining solution is to raise the job's base income to the threshold and increase weekly hours.

Prediction 2 (Gain Coverage): If the job is not bunched and it is very costly to make the job hourly (i.e. $2F > C$), then the firm would keep the job as salaried, and change its base income to $w_2 = \frac{w_0 - \alpha C + (1-\alpha)(V_n^{OT} - V_n)}{1 + \eta_{(w,h,S)}}$ to adjust for the overtime premium, the loss in surplus from the monitoring costs, and the change in the worker's outside option.

Prediction 3 (Reclassification): If the job is not bunched and the cost of reclassifying is smaller than the monitoring cost (i.e. $0 < 2F \leq C$), then the firm would reclassify the job as hourly. Its base income becomes $w_2 = \frac{w_0 - 2\alpha F + (1-\alpha)(V_n^{OT} - V_n)}{1 + \eta_{(w,h,S)}}$.

For a given worker type a, the sign and magnitude of the change in the worker's outside option, $V_n^{OT} - V_N$, depends on the distribution of (θ, F) and the proportion of matches affected by each of the above three responses. If all matches are reclassified or gain coverage, then $V_n^{OT} - V_N < 0$ since workers do not value their pay classification but the added cost to the employer reduces workers' weekly earnings. On the other hand, if all matches are bunched, then $V_n^{OT} - V_N > 0$ if and only if the worker values the increase in earnings more than the loss in leisure. This implies that the base and gross income of reclassified and newly covered employees can either increase or decrease, depending on the value of $V_n^{OT} - V_N$ and the worker's hours of work.

[17] Instead of a fixed cost, one can also allow the monitoring costs to vary by job without affecting the predictions. For example, I can model the relative benefit of being salaried as $F = B - C$ and the monitoring costs as ρC where $0 < \rho < 1$.

Define a job's total surplus as the sum of the firm's profits and the worker's surplus: $T = J + V_e - V_n$. If both the firm and worker accept a job offer, then the total surplus of the job must be positive. One can show that in the benchmark model, the total surplus at the acceptance cutoff $\theta^*(a, F)$ is equal to zero. By introducing the overtime exemption threshold with monitoring costs, the total surplus of salaried jobs with base pays below the threshold decreases.[18] Given a continuous distribution of (θ, a, F), there exist matches close to the cutoff that would be accepted in the benchmark model, but result in a negative surplus in the model of overtime with monitoring costs. These jobs, which are no longer incentive compatible for either the firm or the worker, are dissolved. This gives a forth prediction of the effect of expanding overtime coverage for salaried workers:

Prediction 4 (Employment Loss): Firms and workers no longer accept some jobs with poor match quality (i.e. θ is small) that would have been accepted if there was no overtime coverage.

The above argument also implies that if all jobs have no rents (i.e. the total surplus is zero), then the only response to raising the overtime exemption threshold is a decrease in employment. This result holds for a wide class of labor demand models where firms pay workers their individual marginal product. In these models, the policy will elevate the marginal cost of the worker above their marginal product, leading the firm to layoff the worker. If there are reclassification or bunching effects, then either firms are receiving rents or the marginal product of each worker depends on the number of workers employed within the firm.

While the overtime model with monitoring costs predicts no real labor market effects on hourly workers, I show in appendix C that by introducing wage rigidity, the model generates incentives to decrease the weekly hours of both hourly and salaried workers with overtime coverage.[19] This nests a key prediction of the classic labor demand model and fits the empirical observation that there is a spike in the hours distribution at 40 hours per week (Ehrenberg, 1971). Furthermore, even without monitoring costs, the model with downward nominal wage rigidity generates qualitatively similar predictions to the four discussed

[18] Intuitively, the Nash bargaining solution in the model with monitoring costs is also feasible in the benchmark model, but not optimal. Total surplus is maximized at the Nash bargaining solution.

[19] In contrast to the methods developed in the search literature to generate wage rigidity (see Rogerson and Shimer (2011) for review), I abstract from modeling the cause of wage rigidity and focus specifically on its effects by exogenously imposing that $\frac{w}{h} \geq \frac{w_0}{h_0}$ for hourly workers and $w \geq w_0$ for salaried workers.

above. To avoid the cost of overtime, the worker and firm either no longer agree upon an employment contract, or agree to bunch at the threshold, reclassify pay status, or cut hours. However, under the wage rigidity model, only employees initially working above 40 hours per week are affected. Given that I do not observe salaried workers' hours (see section 4), credibly distinguishing between the model with monitoring cost and the model with wage rigidity is beyond the scope of this paper. Instead, I use the predictions of these models to guide my empirical analysis.

3.3 Labor Market Dynamics with Endogenous Contract Rates

Following the conventional approach in the macroeconomics literature, I endogenize the job match creation rate by modeling the firm's decision to create vacancies. Let v be the number of vacancies per worker in the labor force, and u the unemployment rate. Define market tightness as $k = \frac{v}{u}$. Suppose the job match rate follows a constant returns to scale technology

$$m(u, v) = vq(k)$$

where $q(k) = m(\frac{u}{v}, 1)$ is the vacancy filling rate from the perspective of the firm. The job arrival rate (λ in the previous subsection) from the perspective of the worker is $\frac{m(u,v)}{u} = kq(k)$.

Each employer can create a vacancy at a cost $\psi > 0$. The expected value of creating a vacancy, J_v, is characterized by

$$rJ_v = -\psi + q(k)\sigma(\Phi)(J_F - J_v)$$

where $\sigma(\Phi)$ is the probability that a match is accepted by both parties,[20] and J_F is the expected value of a filled vacancy. Suppose that prior to the announcement of the overtime policy, the labor market is in steady state where employers created vacancies until $J_v = 0$. After the announcement of the policy, the expected value of a match $\sigma(\Phi)J_F$ decreases, so that $J_v < 0$.[21] In response, firms reduce the number of vacancies, v, until $J_v = 0$.

To characterize the dynamics of u, I assume that the job loss rate equals the job finding rate prior to the announcement of the policy: $\delta(1 - u) = kq(k)\sigma(\Phi)u$. This implies a steady

[20] In other words, it is the measure of the set $\Phi = \{(\theta, F, a) | \theta \geq \theta^*(F, a)\}$

[21] Since firms are forward looking, the value of $\sigma(\Phi)J_F$ decreases immediately following the policy announcement, and will continue to decrease until the date that the policy goes into effect.

state unemployment rate of

$$u = \frac{\delta}{\delta + kq(k)\sigma(\Phi)}$$

The policy reduces the number of vacancies and the probability that a match is accepted, so the unemployment rate increases. Since firms and workers are forward-looking, the steady state adjusts immediately following the announcement of the policy:

Prediction 5 (Forward Looking): There will be fewer new hires of salaried workers earning between the old and new thresholds following the announcement of the new overtime exemption threshold, even before it goes into effect.

Intuitively, this prediction holds even if the job destruction rate (i.e. δ) is an endogenous decision of the firm and incumbent workers have firm-specific human capital. Since layoffs are instantaneous, the firm would not layoff any workers until the policy goes into effect. Between the announcement of the policy and the date that it goes into effect, the firm can either continue hiring workers at the same rate as before, then fire them when the policy becomes binding, or reduce its hires to avoid the vacancy cost. Given large enough vacancy costs, firms would choose the latter and I would expect to observe a reduction in hires immediately following the announcement of the policy. The model also predicts no effect on layoffs in response to the December 2016 policy since it was never binding.

4 ADP Data

I use anonymized monthly administrative payroll data provided by ADP LLC, a global provider of human resource services that helps employers manage their payroll, taxes, and benefits. As part of their business operations, ADP processes paychecks for 1 in 6 workers in the United States. Their matched employer-employee panel allows me to observe monthly aggregates of anonymous individual paycheck information between May 2008 and January 2020. The data contains detailed information on each employee's salaried/hourly status, income, hours, pay frequency (i.e. weekly, bi-weekly, or monthly), sex, industry, and state of residence. Given that the overtime exemption threshold varies across states, I partition the sample by workers in California, New York, Maine, Alaska, and the rest of the United States (henceforth called FLSA states). In my analysis of the 2016 federal policy, I restrict the sample to workers in the 46 FLSA states.

A significant advantage of the ADP data over commonly used survey data or other administrative datasets is that it records each worker's standard rate of pay as of the last paycheck in the month, separate from other forms of compensation and without measurement error. This enables me to calculate precisely the measure of weekly base pay that determines employees' exemption status. For salaried workers, the standard pay rate is the fixed salary they receive per pay-period irrespective of their hours or performance. Following the Department of Labor's guidelines, I compute salaried workers' weekly base pay as the ratio between their salary per pay-period and the number of weeks per per-period.[22] For hourly workers, the standard pay rate is simply their wage. As a simple benchmark to compare the weekly base pay and hourly wage of workers who transition between salaried and hourly status, I define the base pay of hourly jobs as 40 times the wage.[23]

The other key measures of income in the data are employees' monthly gross pay and monthly overtime pay.[24] For a given worker-month, the gross pay variable is defined as the total pre-tax compensation paid over all paychecks issued to the worker in that month. To express gross pay and overtime pay in the same denominator as base pay, I normalize them to the weekly-level following a procedure described in detail in Appendix D. While the ADP data also has a variable for the total number of hours worked per month, employers only accurately record this information for hourly employees. The hours of salaried workers are often either missing or set to 40 per week. Since employers do not have to keep track of salaried workers' hours, this limitation is likely endemic to all administrative firm datasets, and not just the ADP data.

Motivated by the job search model in section 3, I aggregate the data by firm, month, pay classification, and bins of base pay. To distinguish workers in the "treated" interval from those above it, I set the new threshold as the left end point of a bin in all my specifications. I top code base pay at $\$2,500$ per week. Collapsing the data in this way enables me to measure the effect of the policy along the entire distribution of base pay, separately for salaried and hourly jobs. The main outcome variables in my analysis of the employment and income

[22]For example, a salaried worker with a statutory pay of \$3000 per month would have a weekly base pay of $\$3000 * \frac{12}{52}$=\$692.31.

[23]This is analogous to the common approach in the minimum wage literature to define a salaried worker's wage as their weekly earnings divided by 40.

[24]As discussed in Appendix D, I impute overtime pay from a variable that often reports overtime, but may also include other forms of compensation. Moreover, since firms are not required to separately report overtime from gross pay, the imputed monthly overtime pay underestimates the total amount of overtime paid in the economy.

effects are the number of workers, total base pay, total overtime pay, and total gross pay within each classification-bin. In the current version of the draft though, I will only examine the effects of the policy on the first two outcomes: employment and base pay. I will update the draft with the other outcomes in future revisions.

Leveraging the matched employer-employee panel structure of the data, I also measure the flow of workers into, out of, and within firms. Between any two months, I can categorize workers into either stayers, new hires, or separations. Among stayers, I further partition the sample by workers who switched pay classifications and those who had the same salaried/hourly status in both months. Collapsing each of these subsamples by firm-classification-bin, I construct the frequency distributions for stayers, new hires, separations, reclassifications, and non-reclassified workers. The effect of the policy on the distribution of each of these subsamples identifies the specific mechanisms that firms use to adjust to the policy.

I make three restrictions to the sample of firms in my analysis. First, the entry and exit of firms in the data reflect both real business formations and the decision of existing firms to partner with ADP. I find that the flow of firms into the ADP sample deviates from the Business Formation Statistics published by the US Census.[25] To prevent the sample selection from affecting my estimates, I restrict my main sample to a balanced panel of firms between April and December of each calendar year in my analysis of the 2016 FLSA policy, and within six months of each event in the event-study analysis. I show in the appendix that my results are robust to using an unbalanced panel. Second, I drop the largest 0.1% of firms within each year since shocks to these businesses have a disproportionately large influence on the results of my firm-level analysis.[26] Third, ADP offers two payroll products, one designed for firms with at least 50 employees and one for smaller firms. The monthly payroll data that I use in my main analysis is derived from the former and is therefore by construction, restricted to businesses with 50 or more employees.

5 Evidence that Firms Responded to the Policy

In this section, I present evidence that although the 2016 policy was never legally binding, companies nevertheless responded to the proposed overtime exemption threshold. In figure

[25] For a detailed analysis of the representativeness of the ADP data, refer to (Grigsby *et al.*, 2019).

[26] This restriction drops 58 firms in 2016, accounting for 11.6% of all workers in the sample that year. I discuss in appendix G the trade-off to dropping large firms.

2a, I overlay the frequency distribution salaried workers' base pay in April 2016 and December 2016, where the frequency in each bin is averaged over the balanced panel of firms that are observable in both months. Reviewing the graph from left to right, four features stand out.

First, there are very few workers below the old threshold of \$455 per week and a noticeable spike in the distribution at exactly the old threshold in both months. Second, there was a decrease in the number of workers with base pays between the old and new thresholds between April and December. The average firm employed 13.19 salaried workers with base pays between \$455 and \$913 in April 2016, and only 10.47 such workers in December 2016 - a decrease of 23%. Third, there is a large spike in the distribution at the $[913, 933)$ bin that appears in December but not April. Forth, there is employment growth above the new threshold, concentrated at regular intervals in the distribution. These recurring spikes along the entire distribution correspond to annual salaries at multiples of \$5000.

These features are even more evident in figure 2b where I plot the difference between the two distributions in figure 2a. As a placebo check, I also overlay the difference-in-distributions between April and December of each year from 2012 to 2015. Consistent with prediction 1 of the job-search model, firms bunched workers' base salaries at the new \$913 overtime exemption threshold in 2016 but not in any of the four preceding years. Contrary to prediction 2, I do not observe any increase in the number of workers to the left of the old threshold that would suggest that newly covered workers' base pay decrease to negate the costs of overtime and monitoring hours. This potentially suggests that firms face downward nominal wage rigidity constraints, or the policy significantly increased the value of continued searching. I explore predictions 1 and 2 more closely in section 6.3.

Replicating the same analysis for hourly workers, figure 2c depicts the frequency distribution of hourly workers' base pay in April and December 2016. Compared to salaried workers, there are nearly twice as many hourly workers and the distribution of their base pay is more right-skewed. To distinguish the effect of the policy from natural employment growth, I compare the change in hourly employment in 2016 to its growth in previous years. Figure 2d plots the difference in hourly employment, by base pay, between April and December of each year from 2012 to 2016. Consistent with prediction 2 that firms reclassify newly covered workers from salaried to hourly, I find that the number of hourly jobs earning between \$455 and \$913 increased more in 2016 than any previous year.

As further evidence that the changes in the frequency distribution of salaried workers' base pay reflect a behavioral response to the nullified policy, I examine its evolution over time. Figure 3 plots the salaried distribution of each month in 2016 and 2017, subtracted by the distribution in April 2016, divided by the total number of firms in the superset of both months. For example, the December 2016 graph in Figure 3 is similar to the blue line in Figure 2b, but includes firms that are only observable for one month. I find that the timing of the growth and decay of the spike at $913 corresponds precisely with the history of the FLSA policy. After the announcement of the policy in May 2016, firms start reducing the number of salaried employees between the old and new thresholds, and bunching workers at the new threshold. This bunching experiences a large increase in December 2016, which is when the new threshold was supposed to go into effect. Since the new threshold was not binding, firms slowly stopped bunching workers at the threshold after January 2017. I show in Appendix figure A.3 that this behavior did not exist between April and December 2015.[27]

6 Aggregate Employment and Income Effect

6.1 Empirical Strategy

Following recent advancements in the minimum wage literature, I identify the aggregate employment effects of raising the OT exemption threshold by first estimating its effect on each bin of the frequency distribution of weekly base pays, and then integrating these effects across all bins (Cengiz *et al.*, 2019; Harasztosi and Lindner, 2019; Derenoncourt and Montialoux, 2019). I estimate the impact of the policy on the distribution of salaried and hourly jobs separately since the predicted effect of raising the overtime exemption threshold differs significantly between these two distributions. In my analysis, I treat the frequency distributions within each firm as an independent observation and cluster estimates at the firm-level.[28]

My empirical strategy stems from the observation that the shape of the difference-distributions in figure 2 are remarkably similar in each year prior to 2016. This suggests that the difference-distribution in 2015 is a reasonable approximation for how the distribution in 2016 would have evolved if not for the new overtime exemption threshold. To control for year-specific employment effects, I apply a linear transformation to the difference-distribution

[27]Unlike the effect of the policy on the salaried distribution, its effect on hourly jobs is more difficult to graphically observe from the evolution of the frequency distribution of hourly workers over time (see Appendix figure A.4).

[28]The effect of the policy on the national distribution is equal to its effect on the average firm, times the number of firms in the population.

in 2015 so that the counterfactual employment growth for jobs paying well above the new threshold closely matches the observed change in employment in 2016.

Formally, let n_{ijkmt} be the number of workers employed at firm i, with pay classification j and base pay in bin k, during month m of year t. I model the number of workers within each firm-classification-bin in December of year t as follows:

$$n_{ijk,Dec,t} = n_{ijk,Apr,t} + \alpha_{jkt} + \beta_{jk} \cdot D_{t=16} + \varepsilon_{ijkt} \tag{2}$$

where α_{jkt} represents the average change in the number of workers with classification j and bin k between April and December of year t, absent the policy. The variable $D_{t=16}$ is a dummy variable for the year 2016 and the coefficient β_{jk} is the causal effect of increasing the overtime exemption threshold on the number of workers in classification-bin jk.

To separately identify the β_{jk}'s from the α_{jkt}'s, I make two modeling assumptions:

$$\beta_{jk} = 0 \text{ for every } k \geq k^*$$

$$\alpha_{jkt} = \gamma_1 \alpha_{jk,t-1} + \gamma_0$$

The first assumption states that the policy has no effect on the number of workers earning above a cutoff bin k^*. This claim follows immediately from the theoretical model if the policy has little effect on workers' value of continued searching. Under this assumption, any change in employment at the top of the distribution reflects only the effect of economic forces unrelated to the new overtime exemption threshold. A naive approach would be to conduct a difference-in-difference analysis using high income jobs as a control group. This strategy would be valid if employment grows equally across the base pay distribution (i.e. $\alpha_{jkt} = \alpha_{jt}$). However, this common trend assumption is inconsistent with the data. Figures 2b and 2d show that the magnitude of the changes in employment vary along the distribution within each year.

To model the heterogeneity in employment growth across base pay, the second condition assumes that the distribution of changes in employment between April and December is similar across years, up to a linear transformation. This assumption is supported by the observation in figure 2b that the spikes in the difference-distribution are concentrated in the same bins each year, and the magnitude of the spikes in any given year are either consistently larger or consistently smaller than those in 2016.

Under these assumptions, I show in appendix E that an unbiased estimator of β_{jk} for any $k < k^*$ is

$$
\begin{aligned}
\hat{\beta}_{jk} &= \left(\bar{n}_{jk,Dec,t} - \bar{n}_{jk,Apr,t}\right) - \hat{\gamma}_1\left(\bar{n}_{jk,Dec,t-1} - \bar{n}_{jk,Apr,t-1}\right) - \hat{\gamma}_0 \\
&= \Delta\bar{n}_{jkt} - \hat{\gamma}_1\Delta\bar{n}_{jk,t-1} - \hat{\gamma}_0
\end{aligned} \tag{3}
$$

where $\bar{n}_{jkmt}$ is the average n_{ijkmt} across all firms, and $\hat{\gamma}_1$ and $\hat{\gamma}_0$ are estimated from

$$
\Delta\bar{n}_{sal,kt} = \gamma_1\Delta\bar{n}_{sal,k,t-1} + \gamma_0 + \epsilon_{sal,kt} \tag{4}
$$

using only salaried workers with bins $k \geq k^*$. I restrict the sample to only salaried workers when estimating equation 4 since changes in employment in the right tail of the hourly distribution, where there is very little mass, reflect more noise than aggregate employment fluctuations. To estimate equations 3 and 4, I apply the Delta method to the estimates of the mean employment across firms that I compute from the following regression:

$$
n_{ijkmt} = \sum_{j,k,m,t} \lambda_{jkmt} D_{jkmt} + \epsilon_{ijkmt}
$$

where D_{jkmt} is an indicator for pay classification j, bin k, month m and year t.

To develop an intuition for equation 3, notice that if $\hat{\gamma}_1 = 1$ and $\hat{\gamma}_0 = 0$, then the treatment effect of the policy is simply a difference-in-difference using the year prior to the policy as the control group. On the other hand, if employment growth in year $t-1$ is uninformative about the growth in year t (i.e. $\hat{\gamma}_1 = 0$), then $\hat{\gamma}_0$ is the average employment growth at the top of the distribution in year t. In that case, equation 3 is a difference-in-difference between low and high income jobs within the same year. My estimator nests both these models, and selects the combination of the two that best predicts the change in employment at the upper tail of the base pay distribution in year t. To test whether this methodology generates a reasonable counterfactual for the change in the distributions absent the policy, I run a series of placebo tests by estimating equation 3 using each pair of adjacent years from 2011 to 2015. Since the policy did not occur prior to 2016, the estimates of the β_{jk}'s in these placebo tests should be close to zero.

In practice, when evaluating the effect of the policy on the salaried distribution, I find that the placebo tests perform better if I assume $\gamma_1 = 1$ and $\gamma_0 = 0$ for base pays below \$625 in the salaried distribution.[29] I therefore impose this restriction in my preferred specification. I choose bins of size $\$96.15 = \frac{5000}{52}$ because the spikes in the salaried distribution occur in

[29] This is consistent with the observation in figure 2b that the difference-distribution exhibits little variation across years in the left tail of the distribution.

intervals of annual salaries of \$5000. I select a cutoff $k^* = \$1778$ where I use the 9 bins greater than or equal to k^* to estimate equation 4. A benefit of selecting a large k^* is that it allows me to test the accuracy of the model by seeing whether it eliminates the spikes between the new threshold and j^*. As described in section 4, I also restrict the sample to a balanced panel of firms within each year. I show that my results are robust to each of the above specification choices in appendices A.6 and A.7.

I estimate the aggregate employment effect of increasing the overtime exemption threshold by summing the effect across all bins less than $k^* = 1778$ in both the salaried and hourly distributions: $\Delta N = \sum_{j,k} \hat{\beta}_{jk}$. I estimate the total effect on the wage bill in a similar fashion: $\Delta W = \sum_{j,k} \hat{\beta}^w_{jk}$, where $\hat{\beta}^w_{jk}$ is estimated from equation 3 by replacing the outcome variable with the total earnings paid to all workers in classification-bin jk. To interpret the magnitude of these effects, I scale the total change in employment and the total change in the wage bill by the number of salaried workers between the old and new thresholds in April 2016, which I denote by $N^s_{Apr,16}$.[30]

$$\Delta\text{Jobs per Affected Worker} = \frac{\Delta N}{N^s_{Apr,16}}$$

$$\Delta\text{Pay per Affected Worker} = \frac{\Delta W}{N^s_{Apr,16}}$$

Another useful statistic is the change in the wage bill for each job lost or gained, which I define as the ratio of the cumulative wage and employment effects: $\Delta\text{Pay per Job} = \frac{\Delta W}{\Delta N}$. As this value approaches zero, firms behave as if their total wage bill is budget neutral with respect to employment. That is, if they lose one worker, they transfer the cost savings onto the remaining workers.

6.2 Aggregate Employment Effect

I start my analysis by estimating the effect of the raising the overtime exemption threshold on the frequency distribution of salaried workers. I plot in figure 4a the bin-by-bin treatment effects estimated from equation 3, and the integral of these treatment effects over the entire distribution. By construction, the identification strategy minimizes the magnitudes of the treatment effects above \$1778. However, it also eliminates the spikes in the distribution

[30] Since some workers between the old and new threshold were already covered for overtime, another useful statistic is to normalize the employment effects by the number of workers who never received overtime prior to April. I will report this is an updated draft of this paper.

above \$913, where the policy is unlikely to have a large effect.[31] This suggests that the econometric model successfully removes confounding effects unrelated to the overtime exemption threshold. Examining the integral of the bin-specific treatment effects, I find that the large drop in the number of workers between the old and new threshold exceeds the spike in the number of workers above the new threshold.

As a placebo check, I estimate equation 3 using adjacent years of data between 2011 and 2015, and plots their respective integrals in figure 4b. For comparison, I also plot the integral of the causal effect in 2016. The placebo checks estimate relatively small effects in every year prior to 2016, indicating that the counterfactual generated by the econometric model closely resembles the observed distribution in December of each of those years. To test the robustness of my results, I also compute the cumulative sum of the treatment and placebo effects using smaller bin widths (figure A.6a), the same values of γ_1 and γ_0 below \$625 as above (figure A.6b), an unbalanced panel of firms (figure A.6c), and different cutoffs j^* above which I assume $\beta_j = 0$ (figure A.6d). I find similar results in all cases, though the integrals of the placebo tests differ from zero when using an unbalanced panel.

I present the estimates of the effect of the policy on the number of salaried workers in column 1 of table 1. To distinguish the effect of the policy on different segments of the distribution, I aggregate the bin-by-bin effects over four distinct intervals: base pays that were always covered for overtime [0,432); base pays that would gain coverage under the new threshold [432,913); base pays right at the new threshold [913,1009); and base pays above the new threshold but below the assumed cutoff for zero treatment effects [1009,1778). By construction, the estimated effect in the omitted interval, [1778,2500), is small.

Raising the overtime exemption threshold decreased the number of jobs between the old and new thresholds in the average firm by 2.481 (s.e. 0.085), and increased the number of jobs bunched at the new threshold by 0.739 (s.e. 0.030). There is also a statistically significant spillover effect above the new threshold that reduced the number of workers by 0.171 (s.e. 0.075). In total, there is a net decrease of 1.955 (s.e. 0.141) salaried positions over the entire salaried distribution. Relative to the number of workers in the bins directly targeted by the policy (13.19), the cumulative change in the salaried distribution represents a 14.8% (s.e. 1.1%) reduction in the affected population.

In column 1 of table 1, I also report the effect of the raising the overtime exemption threshold on the total wage bill paid to salaried workers.[32] Qualitatively, these effects mimic

[31] This is clearer in figure A.5 where I estimate the treatment effect using bin widths of \$20.

[32] The bin-by-bin treatment effects and placebo tests are available in appendix A.8.

the effect on the number of salaried workers. The policy had negligible effects below the old threshold. It reduced the wage bill paid to workers between the old and new threshold by \$1811.33 (s.e. 61.80), increased the wage bill at the new threshold by \$686.43 (s.e. 28.60), and decreased the wage bill paid to workers between the new threshold of \$913 and \$1009 by \$185.44 (s.e 104.75). In total, the wage bill paid to salaried workers decreased by $\$1,247.58$ (s.e. 140.54).[33]

Figure 5a shows the effect of the policy on the number of hourly workers within each \$96.15 bin of weekly base pay. Firms decreased the number of hourly workers in the bin immediately below the old threshold, and increased the number of workers between \$432 and \$1009. In total, there is a net increase in the number of hourly workers, but it is less than the decrease in the number of salaried workers.

To test whether the econometric model described in section 6.1 identifies an appropriate counterfactual to the observed distribution of hourly workers, I use the model to estimate the effect of the policy on the frequency distributions of hourly workers in the four years prior to 2016.[34] I show in figure 5b that the large increase in the number of hourly workers between the old and new threshold is not present in any of the other years. However, the large dip in employment right below the old threshold exists in 2014. Furthermore, this large dip is highly sensitive to the inclusion of the largest 0.1% of firms (see appendix G), and it coincides with the minimunm wage of multiple states(\$8.4 10.6). To explore this further, I show in appendix A.7 that the dip is significantly smaller once I restrict the sample to only states that are only bound by the federal minimum wage (\$7.25). Given that the drop in low-income hourly employment is very sensitive to different sampling selections, I cannot conclusively estimate the exact effect of the policy on hourly employment. However, I am confident that there was a net increase in hourly workers, particularly in the range between the old and new thresholds. In a subsequent revision of this draft, I plan to present other specifications to model the counterfactual hourly distribution, including using the hourly distribution from firms that had no salaried workers affected by the 2016 policy as a control group.

I summarize the effect of the overtime exemption threshold on the number of hourly

[33] A graphical representation of the cumulative effect on the wage bill, along with placebo and robustness checks, are available in Appendix A.8.

[34] The analogous bin-by-bin treatment effects and placebo tests for the base-pay weighted distribution of hourly workers are available in appendix A.9. The results are qualitatively similar to their non-weighted counterparts.

workers and their total wage bill in column 2 of table 1. In response to the policy, the average firm decreased the number of hourly workers with base pays below the old threshold by -0.95 (s.e. 0.232), increased the number of workers between the old and new thresholds by 1.686 (s.e. 0.251) and increased the number of workers within \$96 above the new threshold by 0.21 (s.e. 0.041). These estimates translate to an additional 0.079 (s.e. 0.032) hourly workers for each salaried worker directed affected by the new threshold. Qualitatively, the wage bill effects follow a similar pattern as the employment effects. In total, there was a net increase of 1.041 (s.e. 0.417) hourly workers and an increase in the total wage bill by $\$1,181.02$ (s.e. 282.02).[35]

Summing the estimates in columns (1) and (2), I report in column (3) of table 1 the aggregate employment and income effects of increasing the overtime exemption threshold. Consistent with prediction 4 of the job-search model, I find a negative employment effect significant at the 10% level. My estimates allow me to rule out, with 95% confidence, any employment effects less than -0.138 or greater than 0.03 for each worker directly affected by the new threshold. However, the entire decrease in employment can be explained by the drop in hourly workers earning right below the old threshold. As described earlier, while my current specification suggests a negative employment effect, I would caution against making any conclusive statements regarding the net employment effect with testing further specifications first.

Despite the significant employment effects estimated in this model, the total weekly wage bill of the average firm only decreases by a statistically insignificant \$5.05 per affected worker, or \$73.19 for each job lost. The change in the total wage bill equals the difference between the raises given to workers who are employed and the labor costs saved from reducing employment. Given that the policy affected salaried workers earning at least \$455 per week, the comparatively small drop in the wage bill per job lost suggests that a large part of the money saved from reducing employment was paid out to workers who remain employed.

6.3 Income Effect on Affected Workers

To directly estimate the effect of raising the overtime exemption threshold on workers' income, I use the matched employer-employee structure of the data to split the sample by new hires, separations, and stayers. By focusing specifically on continuously employed workers, I

[35] Recall that the wage bill is calculated under the assumption that all hourly workers work 40 hours per week.

identify the change in the total wage bill due to an income effect, separate from any changes due to employment. This overcomes a methodological limitation of the minimum wage study by Cengiz *et al.* (2019), which extrapolates the average income effect from changes in the total wage bill by assuming that the wage effect on workers entering and exiting employment is the same as the wage effect on workers who remain employed.[36] While that assumption may be valid for small minimum wage changes, it does not hold in my setting where the predicted effect of the policy varies depending on workers' base income relative to the overtime exemption threshold.

One concern with restricting the sample to workers who remain employed between April and December is that by conditioning on an outcome variable, the stayers in 2015 may no longer be a reasonable counterfactual to the stayers in 2016. For instance, if the policy causes firms to disproportionately layoff low-income workers, then my empirical strategy would compare the evolution of base pays between two groups with different average initial incomes. I show empirically that this selection problem is small and unlikely to significantly bias my estimates of the income effect.

I plot in figure 6 the frequency distribution of salaried workers who separate from their employers between April and December of each year between 2012 and 2016. Although there are more separations in the upper end of the distribution in 2016 than in previous years, there are actually slightly fewer separations below the new threshold. Hence, raising the overtime exemption threshold had little effect on the number of layoffs between April and December. I present evidence in section 7 to show that the decrease in aggregate employment is primarily driven by a decrease in new hires. Since the policy did not significantly affect the probability of layoffs, conditioning the sample on stayers does not bias the difference-in-distribution estimates.

I use the method outlined in section 6.1 to estimate the income effect on job-stayers who are paid by salary in April 2016.[37] I plot the these estimates along the entire base pay distribution in figure 7a. As a placebo test, I show in figure 7b that the econometric model

[36] In their study, the average wage effect is calculated as $\frac{W_{-1}+\Delta W}{N_{-1}+\Delta N} \Big/ \frac{W_{-1}}{N_{-1}} - 1$, where W_{-1} and N_{-1} are the total wage bill and total number of affected workers before the policy change, respectively.

[37] The model performs poorly on the placebo tests when evaluating the effect on hourly job-stayers. The econometric model relies on the distribution being stable over time up to a linear transformation. While this may be true for the total number of hourly workers, it does not hold for the subset of job-stayers. One potential explanation for this is that the turnover of hourly workers exhibits large variability from year-to-year, which in turn affects the variability of job-stayers.

estimates small effects in the years prior to 2016.[38] Consistent with prediction 1 of the job-search model, only workers with base salaries close to \$913 per week are bunched at the new overtime exemption threshold. Raising the overtime exemption threshold caused firms to bunch workers who would otherwise have earned between \$720 and \$913 per week. I do not find any effect on the number of job-stayers earning below \$720 per week. Furthermore, the absence of any positive effects below the new threshold rejects a feature of predictions 2 and 3 that firms would decrease workers' base salaries to nullify the costs of paying overtime.[39]

I report the bin-specific treatment effects and their corresponding pay-weighted estimates, aggregated over four intervals, in column (4) of table 1. As expected, there is a statistically significant movement of workers from between the old and new threshold to above the new threshold. This bunching increased the average firm's total wage bill by \$113.89 (s.e. 17.45). Dividing the increase in the wage bill by the wage bill paid to all salaried workers earning between \$455 and \$913 in April 2016, I calculate that the new threshold raised the average affected worker's income by 1.2% (s.e. 0.2%).

7 Effect on the Flow of Workers Between and Within Firms

The aggregate employment effects imply that firms substitute hourly workers for salaried workers in response to a higher overtime exemption threshold. However, it is unclear whether firms make this transition by laying off their salaried workers and hiring new hourly ones, or by simply reclassifying existing workers. Furthermore, it is uncertain how these mechanisms vary by workers' initial salaries. Just as the positive income effects are concentrated among workers earning close to the new threshold, the employment and reclassification effects may also vary across the distribution of affected workers. In this section, I use the panel structure of the data to measure the specific margins by which firms adjust to the policy proposal in 2016, and how these adjustments vary across the distribution of base pays.

7.1 Decomposing the Net Effect into the Effect on Flows

Between any two months, the number of workers within each firm-classification-bin can change through a combination of three channels: the flow of workers in and out of the firm (i.e. employment), the flow of workers between hourly and salaried status (i.e. reclassifica-

[38] Due to the linear transformation used to generate the counterfactual distribution, the cumulative treatment effects do not sum to exactly zero.

[39] This is the also the key prediction of the compensating differentials model of overtime.

tion), and the flow of workers between different base pays within the same pay classification (i.e. wage adjustment). Let Δn_{ijkt}^{emp}, $\Delta n_{ijkt}^{reclass}$, and Δn_{ijkt}^{within} denote the net flow of workers into firm i, classification j, bin k between April and December of year t via employment, reclassification, and wage adjustment, respectively. I show formally in Appendix F that the difference in the total the number of workers between April and December can be decomposed into the sum of these three factors:

$$\Delta n_{ijkt}^{total} = \Delta n_{ijkt}^{emp} + \Delta n_{ijkt}^{reclass} + \Delta n_{ijkt}^{within}$$

In addition, each of the net flows can be further decomposed into an inflow and an outflow relative to the firm-classification-bin.

Following the methodology described in section 6.1, I estimate the effect of raising the overtime exemption threshold on each net flow variable using equations 3 and 4. As before, I evaluate the fit of the model by estimating the effect of the policy on each of the flow measures in the four years prior to 2016. To minimize the magnitude of the placebo effects, I set different restrictions on the parameters of the linear transformation, γ_0 and γ_1, for each outcome that I analyze. When evaluating the effect of the policy on flows due to wage adjustments within the same pay classification, I follow the strategy described in section 6.1: I estimate γ_0 and γ_1 from equation 4 using bins greater than or equal to $k^* = \$1778$, and I set γ_0=0 and γ_1=1 for all bins less than \$625. To estimate the effect of the policy on net employments flows, I select a cutoff of $k^* = 1586$ and do not restrict γ_0 or γ_1 for any bins.[40] In my analysis of net reclassification flows, I restrict γ_0=0 and γ_1=1 for all bins.[41] Similar to section 6.1, I estimate the effect of the policy on each flow into the hourly distribution using the same γ_0 and γ_1 that I use to estimate the analogous flow into the salaried distribution.

7.2 Estimates of Flow Effects

Figure 8 presents the distributional effects of the overtime exemption threshold on the net employment of salaried workers, net reclassification into the salary and hourly distributions, and the net flow of workers within the salary distribution between April and December 2016. It is evident from figure 8a that there are negative employment effects throughout the

[40]While I select a smaller cutoff for k^* than in section 6.1 to shrink the placebo effects, the estimates of the treatment effects in 2016 are very similar for a wide range of cutoffs. The placebo effects are sensitive to the cutoff because the shape of the distribution of net employment flows exhibits more variation between years compared to the distribution of salaried workers (see figure A.10a).

[41]I show in figure A.11a that the net reclassifications is very stable over time for all bins.

entire segment of affected workers. However, without knowing the employment flows into the hourly distribution, it is unclear from this analysis whether total employment increased or decreased. nevertheless, these employment effects are small relative to the reclassification effects presented in figure 8b. The bulk of the decrease in the number of salaried workers is due to reclassification. Furthermore, the distribution of net reclassifications into the hourly distribution has a very similar shape to the negative of the net reclassifications into the salaried distribution. This suggests that firms are paying their reclassified workers a wage equal to their previous salary divided by 40. The similarity between these two distributions is inconsistent the hypothesis of the contract model that firms would cut workers' base pays in response to the overtime requirement. Lastly, although bunching of new hires accounts for part of the spike at $913, figure 8c shows that the largest share of the spike is due to the bunching of salaried workers with initial base pays between $720 and $913 who remain salaried in December, consistent with the evidence presented in figure 7.

To validate the econometric model used to estimate these flow effects, I test how well it fits the distribution of flows in the years prior to 2016. Although the cumulative placebo effects on net employment flows into the salaried distribution do deviate from zero, these effects are small compared to the effect in 2016 and do not show any systematic bias in either direction (see figure A.10b). I show in appendices A.11b, and A.12b that the policy had negligible effects on net reclassification and wage adjustment flows into the salaried distribution in the placebo years, respectively. Similarly, the model also estimates small reclassification effects into the hourly distribution in the years prior to 2016 (see figure A.14). However, the placebo effects strongly deviate from zero when evaluating the effect of the policy on the net employment and wage adjustment flows into the hourly distribution (see figures A.13 and A.15).[42] I do not present estimates of the policy's effect on flow variables that do not pass the placebo test.

I summarize the effect of the policy on each of the flows into the salary distribution in columns (1) to (3) of table 2. Column (1) reports that 0.032 (s.e. 0.006) salaried jobs were lost to unemployment for each worker directly affected by the new threshold. This is approximately half the size of the aggregate employment effect estimated in section 6. In theory, the sum of the employment flows into the salaried and hourly distributions equals the total change in employment: $\sum_{j,k} \Delta n^{emp}_{ijkt} = \sum_{j,k} \Delta n^{total}_{ijkt}$. While this may indicate that the policy also cut hourly jobs, in practice, the identify may not hold because I use a different

[42]This likely reflects volatility in turnover rates between years. Even if the net number of hourly workers is stable, the number of stayers, hires and separations may not be.

linear transformations to estimate the flow effects and aggregate effects.

Decomposing the net employment flows into hires and separations, I find the the negative effect on net employment flow is primarily driven by changes in firms' hiring decisions. As discussed in section 6.3, the policy had little effect on the number of separations between April and December 2016. In contrast, I show in figure 9 that there are noticeably fewer new hires between the old and new thresholds in 2016 compared to previous years. Furthermore, workers that do get hired are more likely to be bunched at the new threshold compared to previous years. Consistent with prediction 5 of the job search model, this implies that employers are forward-looking and slowed down their hiring of affected workers before the new overtime exemption threshold went into effect. This result is similar to recent findings that firms cut employment in response to the minimum wage via a reduction in hires rather than an increase in layoffs (Gopalan *et al.*, 2018).

Column (2) of table 2 indicates that among the set of workers continuously employed as salaried, raising the threshold decreased the number of workers with base pays between \$432 and \$913 by 0.606 (s.e. 0.028) and increased the number of workers between \$913 and \$1009 by 0.634 (s.e. 0.013).[43] Scaling the bunching at the threshold by the initial number of workers between the old and new thresholds, I find that firms bunched 4.8% of all stayers directly affected by the policy. The total wage bill paid to always-salaried workers increased by \$107.13 (s.e. 19.89), which translates to a 1.5% (s.e. 0.003%) increase in their average base pay. Since jobs that firms choose to reclassify are different from jobs that stay salaried, this effect on average base pay includes selection-bias, albeit small given that the number of always-salaried workers is nearly 8 times that of reclassified workers.

Column (3) of table 2 reports the estimates of the net reclassification effects into the salaried distribution. In response to the policy, the net number of reclassifications into salaried status decreased by -1.318 (s.e. 0.083). This is equivalent to -0.100 (s.e. 0.007) workers for each directly affected worker. Although the choice of γ_0 and γ_1 differ in each application of the econometric model, it is comforting to see that the sum the employment and reclassification effects is not significantly different from the total change in the number of salaried workers. The magnitudes of the estimates also makes it clear that the bulk of the decrease in salaried workers is due to reclassification. A ratio of the reclassification effect

[43]By construction, the change in the total number of always-salaried workers should be zero. However, since the econometric models uses a linear transform of the 2015 distribution to identify a counterfactual distribution, the sum of the difference between the observed and predicted distributions do not necessarily equal zero.

to the total effect suggests that at least 67% of the decrease in salaried workers is due to reclassification.

To understand whether the change in net reclassification is driven by an increase in flows from salaried-to-hourly or a decrease in flows from hourly-to-salaried, I plot the distribution of reclassifications separately by the direction of the flow, and compare these distributions over time. I observe in figure A.11 that the majority of the net reclassification effect is due to firms changing workers from salaried to hourly status. In the years leading up to 2016, most reclassifications were from hourly to salaried, possibly reflecting promotions, but this trend reversed in 2016 with a large increase in the number of workers being reclassified from salaried to hourly.

By reclassifying workers from salaried to hourly, firms reduced the wage bill paid to salaried workers by \$989.31 (s.e. 73.59). In comparison, column (4) of table 2 reports that the reclassifications increased the wage bill paid to hourly workers by \$1008.09 (s.e. 71.21).[44] The net change in the wage bill of all workers in the sample who were reclassified between April and December 2016 is \$18.79 (s.e. 10.48) (see column (5)). In other words, reclassified workers experienced a 0.014% (s.e. 0.008%) increase in their base pay.

7.3 Alternate Specification Check: Difference-in-Difference

In this subsection, I compare my main difference-in-distributions results to estimates from a standard difference-in-difference design where I follow the group of directly affected workers over time. The treatment group in this specification are salaried workers in 2016 with base pays between \$455 and \$913 in April 2016, and the control group are salaried workers in the same income bracket in April 2015.[45] This allows me to test the robustness of my main results to an alternative identification strategy that only assumes that absent the policy, salaried workers in the \$455 to \$913 pay range would evolve similarly in both 2015 and 2016. Unlike the difference-in-distributions approach, this method takes no stance on the evolution of employment across the base pay distribution. However, one limitation of following workers

[44] I count workers whose status transitions from or to missing in my measure of net reclassification flows. This explains why the cumulative number of jobs reclassified into hourly status does not equal the negative of the cumulative number of jobs reclassified into salaried status. Given that the sum of the net reclassifications is less than one, I am underestimating the income effects of the policy for reclassified workers.

[45] Alternatively, one could use salaried workers in 2016 with base pays greater than \$913 as the control group. However, I find that the outcome variables for this group has a significantly different pre-trend than for individuals earnings within the treated interval.

over time is that it misses any effect on the hiring rate, which according to the previous results, is the main source of the negative employment effects among salaried workers.

I estimate the following difference-in-difference specification:

$$y_{qrt} = \alpha_t + \gamma_r + \sum_{\substack{k=-12 \\ k\neq -1}}^{k=12} \beta_k (1[t = 2016]x1[r = k]) + \varepsilon_{qr} \tag{5}$$

where y_{qrt} is the dependent variable for worker q in the r^{th} month since May of year t. A worker is in the treatment group if $t = 2016$, indicating that they earned between \$455 and \$913 in April 2016. I control for a treatment group fixed-effect α_t and event time fixed-effects γ_r. The coefficients of interest are β_k, which measure the average difference in outcome between the treatment and control groups k months from May of their respective sampling years. In my evaluation of the effect of the policy on base pay and salaried/hourly status, I restrict the sample to workers who are continuously employed at the same firm for a year before and after the event date.

I plot the raw time series graphs and the difference-in-difference estimates in appendix H. Figure H.1 shows that salaried workers in 2016 are more likely to be employed at the same firm before April and more likely to leave the firm after April, compared to salaried workers in 2015. While the difference in the survival curves fails the common trend assumption, the linear pre-trend does not appear to be affected by the expectation of the new overtime exemption threshold. This affirms the previous result that the policy had little effects on worker separations.

The difference in the evolution of base pays also exhibits a negative pre-trend prior to April (see figure H.2). Nevertheless, the timing and shape of the base pay effects are clear indications that increasing the overtime exemption threshold had a positive income effect on affected workers. Workers in 2015 and 2016 have similar salaries from January to August, but then from September onwards, the base pay of affected workers begins to rise significantly, culminating with a large jump from November to December, and then remaining relatively stable afterwards. The estimate in December implies that raising the overtime exemption threshold increases the weekly base pay of affected workers by 0.8% (s.e. 0.05%) on a base of \$738.05, which is smaller than the earlier estimate of 1.2% from the difference-in-distributions analysis but does not include spillover effects.

To identify spillover effects, I estimate equation 5 for workers with base pays between \$913 and \$1113 in April 2016, using similarly defined workers in 2015 as the control group.

These two groups do share a common trend in base pay prior to the announcement of the new overtime exemption threshold. I find that the policy raised the weekly base pay of these workers by 0.1% in December 2016 (see appendix H.3).

I also use equation 5 to estimate the effect of the policy on affected workers' probability of being reclassified (see appendix H.4). My estimates imply that the policy increased the probability of a salaried worker in April 2016 becoming hourly in December by 7.6 percentage points (s.e. 0.04 p.p.), from a base of 2% for similarly defined workers in 2015. While the 7.6 p.p increase in reclassifications is smaller than the estimate from the difference-in-distributions approach, it also omits any reductions in the reclassification of hourly workers as salaried. Overall, the difference-in-difference estimates of the employment, income, and reclassification effects on directly affected workers are consistent with the estimates from the difference-in-distribution analysis.

8 Discussion and Conclusion

This paper presents new facts that inform the policy debate on whether or not to expand overtime coverage. Existing cost-benefit projections by the Department of Labor, which draws from previous studies in the literature, are unable to infer the direction of the employment effect. I show in my paper, albeit inconclusively, that the policy potentially decreased aggregate employment.

While the policy may have decreased the number of jobs in the economy, it also increased the average base pay of remaining workers by 1.2% (s.e. 0.2%). This is larger than the DOL's 2016 assessment of the policy, which predicted that the average affected worker would experience a 0.7% increase in their weekly earnings *after* including overtime compensation. In terms of base pay, the DOL calculated that only about 2% of affected workers would be bunched above the threshold, while 18% of workers would experience a 5.3% decrease in their regular rate of pay, as predicted by previous evaluations of the "contract model" of overtime (Trejo, 1991). However, I find no evidence that firms reduced workers base pays in response to being covered for overtime. Instead, I show that the only effect on the distribution of job-stayers is the bunching of some workers who would otherwise earn between \$720 and \$913 per week to above the new threshold. In further contrast to the analysis by the DOL, I find that the policy increased the number of reclassifications from salaried to hourly status by 0.1 (s.e. 0.006) per directly affected worker, whereas the DOL assumed this effect to be negligible. Whether workers prefer to be paid a fixed salary or an hourly wage though, is an open question that deserves further research.

Another question that deserves further attention is the effect of raising the overtime exemption threshold on workers' hours. Unfortunately, this is challenging to observe in administrative payroll data as firms seldom record the hours of salaried workers. To address this issue, I tried estimating the hours effect using self-reported data from the Current Population Survey (CPS). However, due to sampling error, I am unable to even replicate any of the bunching, income, or reclassification effects from the main analysis of the paper (see appendix I). Thus, while survey data may be potentially useful in measuring salaried workers' hours, it is uncertain whether they have the precision to accurately capture the effects of the policy.

Although my paper offers the most comprehensive assessment of the FLSA overtime exemption policy to date, there exist many avenues for future research that are beyond the scope of this study. First, given that this paper examines a policy that was suspended before its inaugural date, it is uncertain whether a binding policy will have a different effect. In particular, it would be valuable to study the long-term impacts of raising the overtime exemption threshold. I will accomplish this in a subsequent revision of this draft that will include a detailed analysis of the state-level changes in the overtime exemption threshold. Second, similar to the minimum wage literature, it is worth exploring the relationship between the depreciation in the overtime exemption threshold over the past 30 years and the growing wage inequality over the same period. Lastly, while this paper does not take a stand on the normative implications of the trade-off between aggregate employment and average earnings, it would be an interesting avenue of research to model the welfare impacts of this trade-off and applying it to the overtime exemption threshold.

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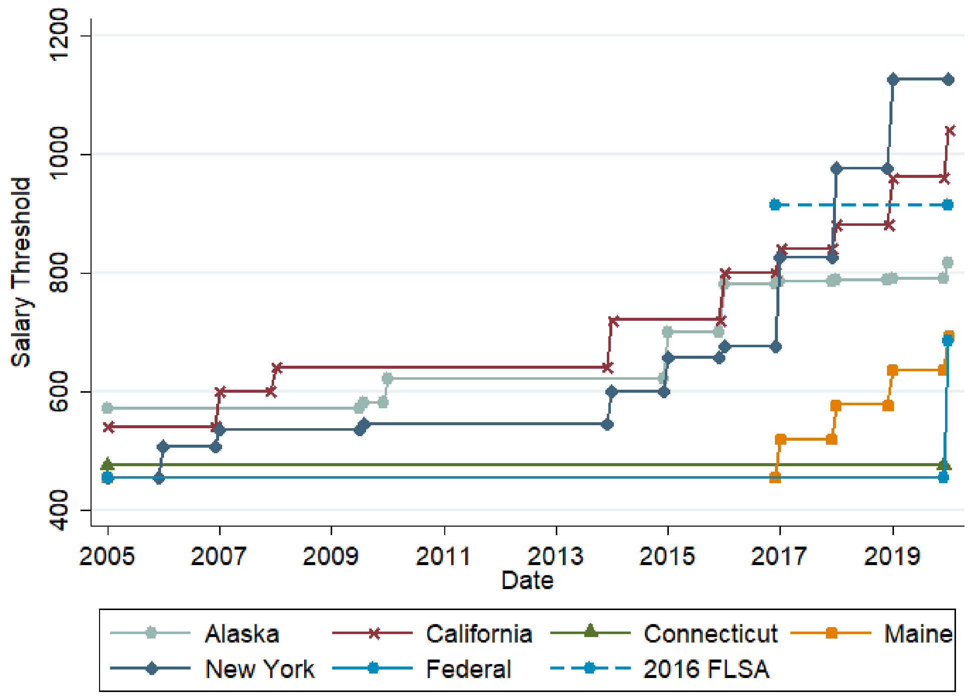
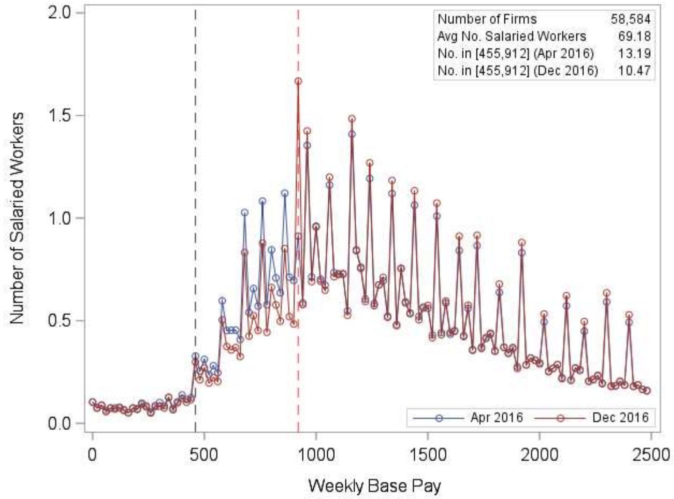
Figure 1: Variation in State-Specific Overtime Exemption Thresholds

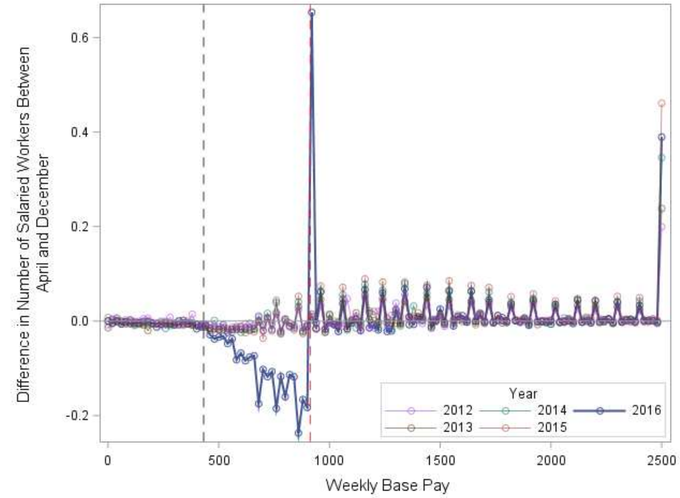


Notes: This figure shows the binding overtime exemption threshold in each state between 2005 and 2018. All states not included in the graph are covered by the federal overtime exemption threshold. The line "2016 FLSA" represents the threshold that was supposed to go into effect on December 1, 2016 but was nullified in November 2016. In Alaska and California, the threshold equals 80 times the state minimum wage. In Maine, the threshold equals 3000/52 times the minimum wage. In New York, the threshold was 80 times the minimum wage up until December 2016. Starting in January 2017, the threshold in New York varies by county and size of employer. I plot the threshold faced by employers with more than 11 employees in New York City, which is the highest threshold in the state.

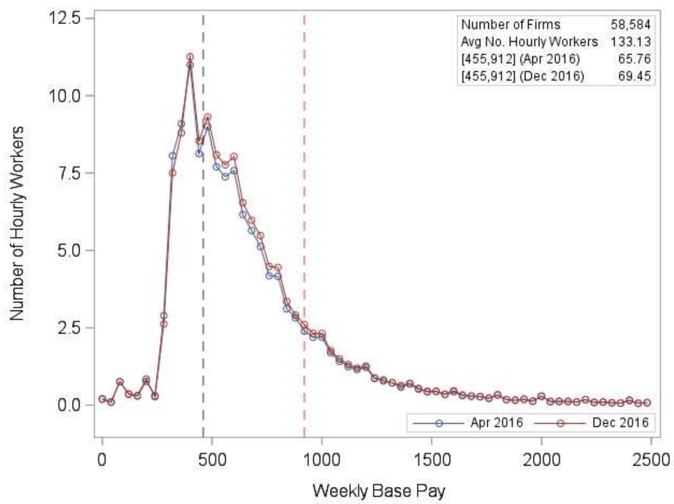
Figure 2: Frequency Distribution of Base Pay, by Pay Classification



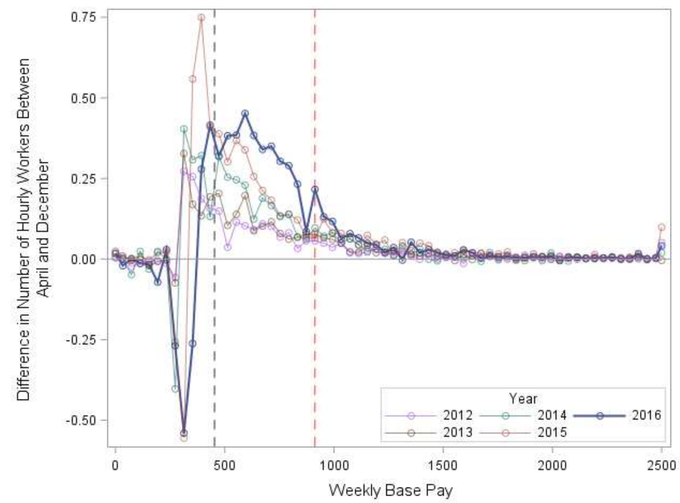
(a) Frequency Distribution of Salaried Workers



(b) Difference in Distribution of Salaried Workers



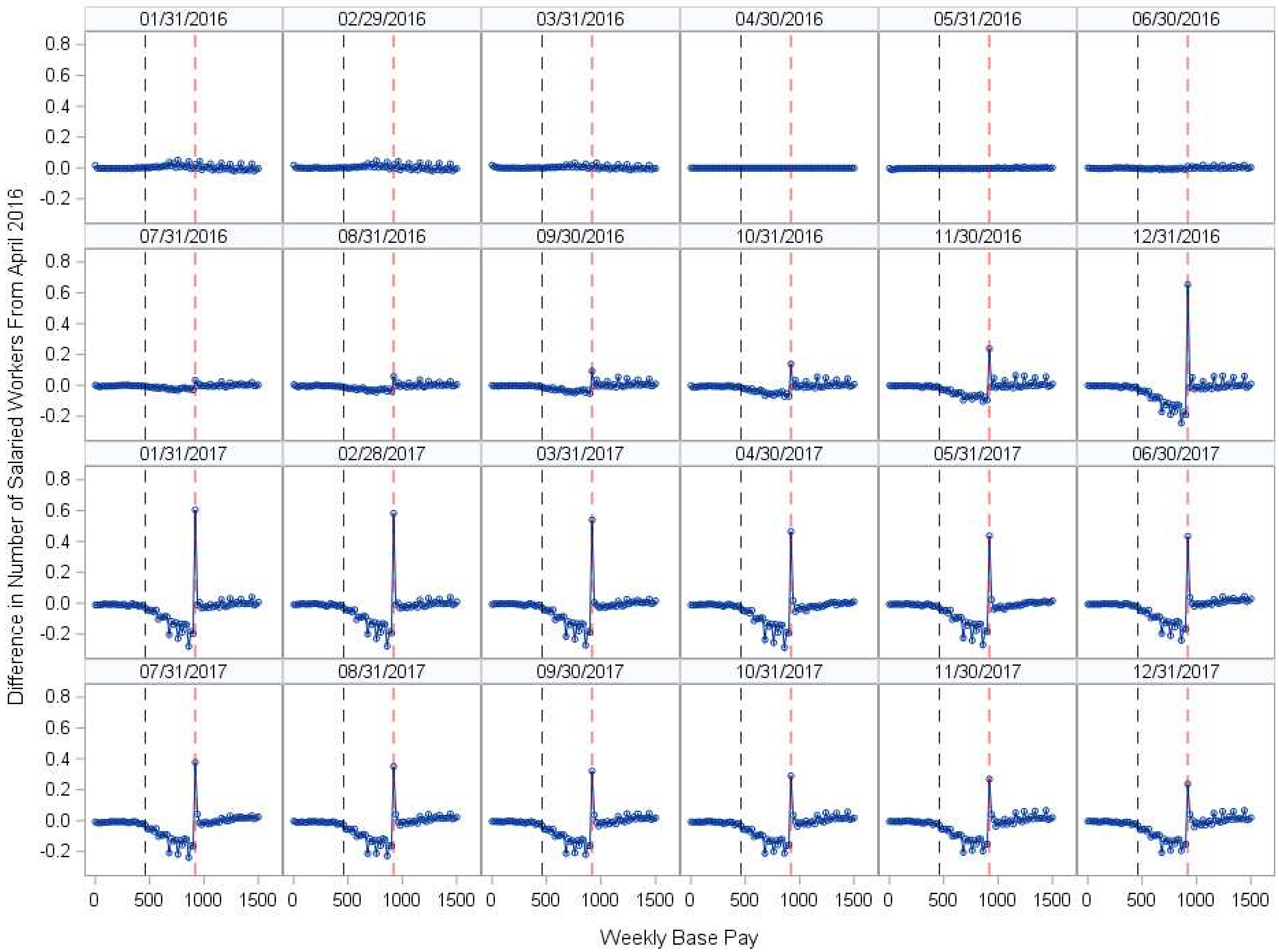
(c) Frequency Distribution of Hourly Workers



(d) Difference in Distribution of Hourly Workers

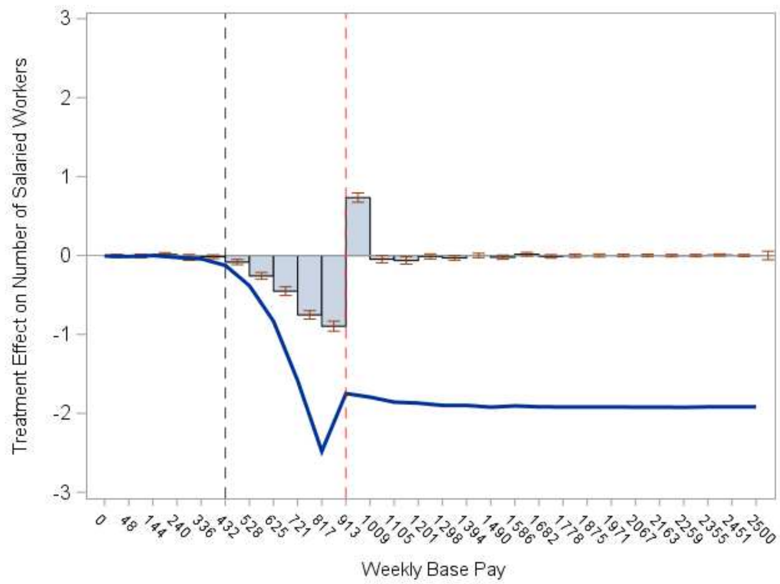
Notes: Panel (a) shows the frequency distribution of weekly base pay (as defined in section 4) of salaried workers in April and December 2016, scaled by the number of firms in the balanced sample. The blue line represents the distribution in April and the red line represents the distribution in December. The first bin has a width of \$12.99. All other bins have width \$20. The distributions are truncated at \$2497. The vertical black dashed line is at the bin containing the overtime exemption threshold in April (\$455), while the red dashed line is at the bin containing the proposed threshold for December (\$913). Panel (b) shows the difference between the frequency distribution of salaried workers' base pay in December and April, by year. The last bin counts all workers with base pays≥\$2513. Panels (c) and (d) are the hourly worker analog to panels (a) and (b), respectively, collapsed over \$40 bins.

Figure 3: Frequency Distribution of Salaried Workers by Month, Differenced by the Distribution in April 2016

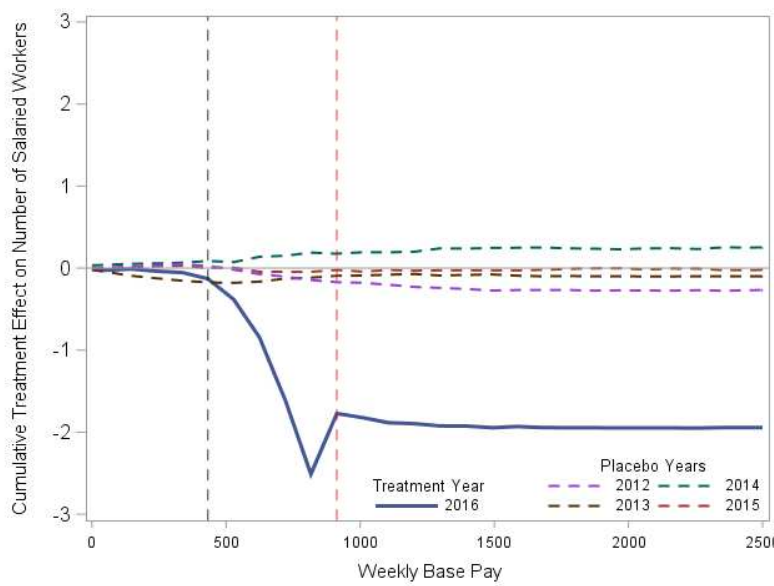


Notes: The figure shows the frequency distribution of weekly base pays in each month of 2016 and 2017, subtracted by the frequency distribution in April 2016. For each month, I scale the distribution by the number of firms in the superset of all firms that appear in the data that month and in April 2016. Within each graph, the bins are \$20 wide except for the first bin which goes from \$0 to \$12.99. The vertical black dashed line is at the bin containing the overtime exemption threshold in April (\$455), while the red dashed line is at the bin containing the threshold (\$913) that was supposed to go into effect on December 1, 2016.

Figure 4: Effect of Raising the OT Exemption Threshold on the Frequency Distribution of Salaried Workers' Weekly Base Pay



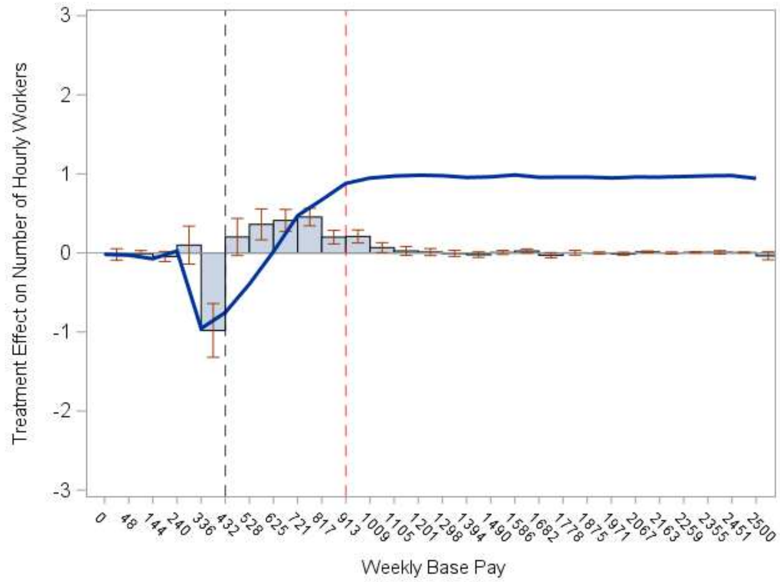
(a) Estimates of the Effect on Number of Salaried Workers



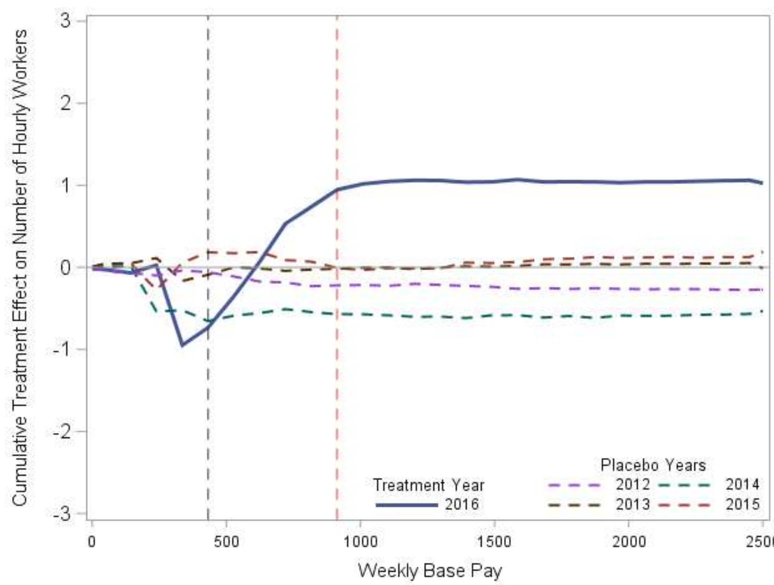
(b) Placebo Test of Effect on Salaried Distribution

Notes: Each bar in panel (a) shows the effect of the policy on the number of salaried workers in each \$96.15 bin in Dec 2016. The treatment effects are estimated using equation 3. The solid blue line is the running sum of these effects. The x-axis ticks denote the starting value of each bin. Panel (b) shows the cumulative effect of raising the OT exemption threshold on the number of salaried workers in December of each year between 2012 and 2016. The solid blue line in panel (b) is the same as the solid blue line in panel (a). The dotted graphs are similarly defined running sums, except the effect of the policy is estimated using the December and April distributions of the labeled year and the preceding adjacent year. In both graphs, the vertical black and red lines are at the start of the bins that contain the old and new OT exemption thresholds (\$455 and \$913), respectively.

Figure 5: Effect of Raising the OT Exemption Threshold on the Frequency Distribution of Hourly Workers' Weekly Base Pay



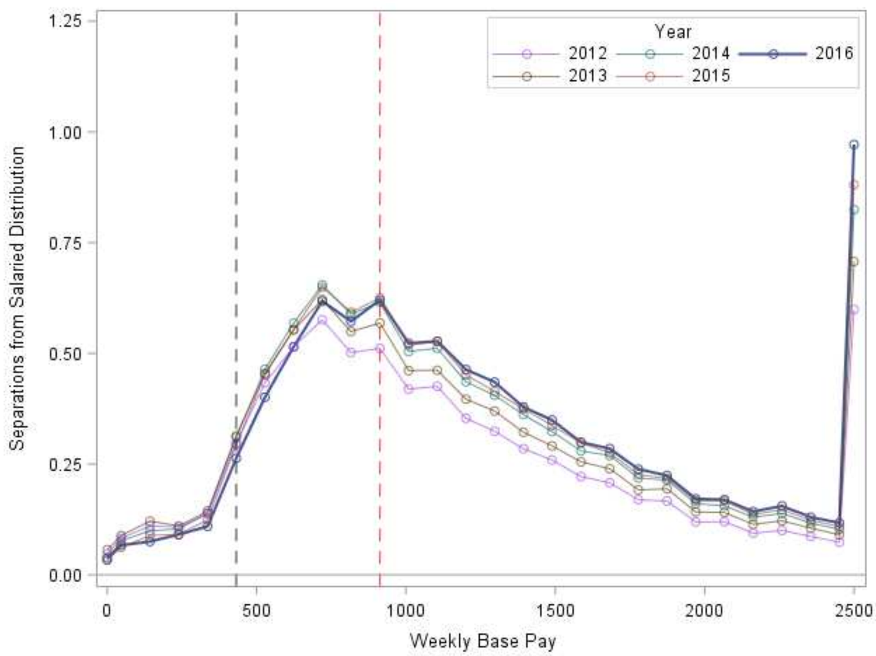
(a) Estimates of the Effect on Number of Hourly Workers



(b) Placebo Test of Effect on Hourly Distribution

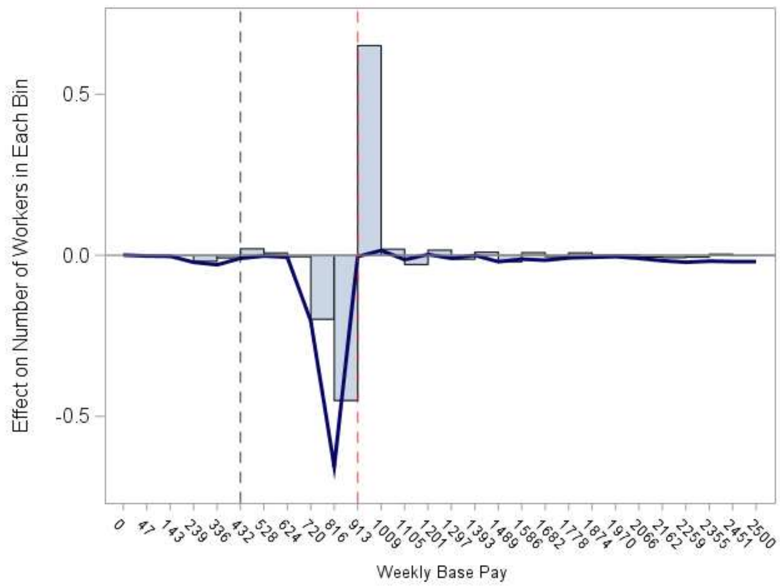
Notes: Each bar in panel (a) shows the effect of the policy on the number of hourly workers in each \$96.15 bin in Dec 2016. The treatment effects are estimated using equation 3. The solid blue line is the running sum of these effects. The x-axis ticks denote the starting value of each bin. Panel (b) shows the cumulative effect of raising the OT exemption threshold on the number of hourly workers in December of each year between 2012 and 2016. The solid blue line in panel (b) is the same as the solid blue line in panel (a). The dotted graphs are similarly defined running sums, except the effect of the policy is estimated using the December and April distributions of the labeled year and the preceding adjacent year. In both graphs, the vertical black and red lines are at the start of the bins that contain the old and new OT exemption thresholds (\$455 and \$913), respectively.

Figure 6: Distribution of Separations Between April and December, Salaried Workers

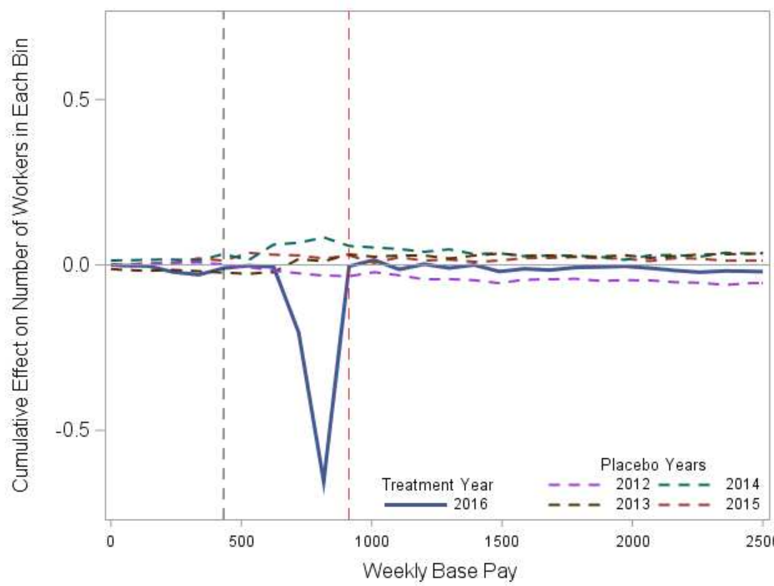


Notes: The figure shows the frequency distribution of base pays in April of each year between 2012 and 2016, averaged across firms, for salaried workers who separate from their employer between April and December.

Figure 7: Effect of Raising the OT Exemption Threshold on the Frequency Distribution of Salaried Job-Stayers



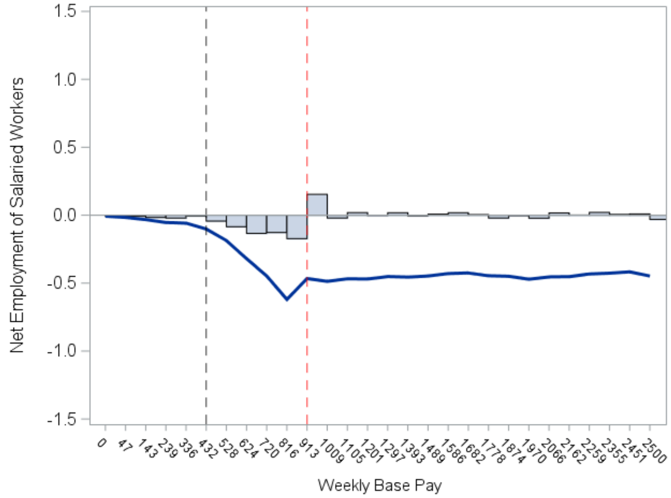
(a) Estimates of the Effect on Number of Salaried Workers



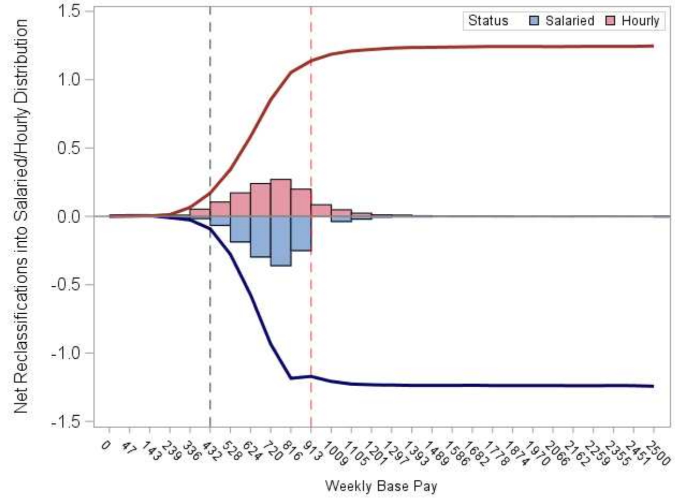
(b) Placebo Test of Effect on Salaried Distribution

Notes: The sample is restricted to workers who are paid by salary in April and are employed at the same firm in both April and December. Each bar in panel (a) shows the effect of the policy on the number of job-stayers in each \$96.15 bin in Dec 2016. The treatment effects are estimated using equation 3. The solid blue line is the running sum of these effects. The x-axis ticks denote the starting value of each bin. Panel (b) shows the cumulative effect of raising the OT exemption threshold on the number of job-stayers in December of each year between 2012 and 2016. The solid blue line in panel (b) is the same as the solid blue line in panel (a). The dotted graphs are similarly defined running sums, except the effect of the policy is estimated using the December and April distributions of the labeled year and the preceding adjacent year. In both graphs, the vertical black and red lines are at the start of the bins that contain the old and new OT exemption thresholds (\$455 and \$913), respectively.

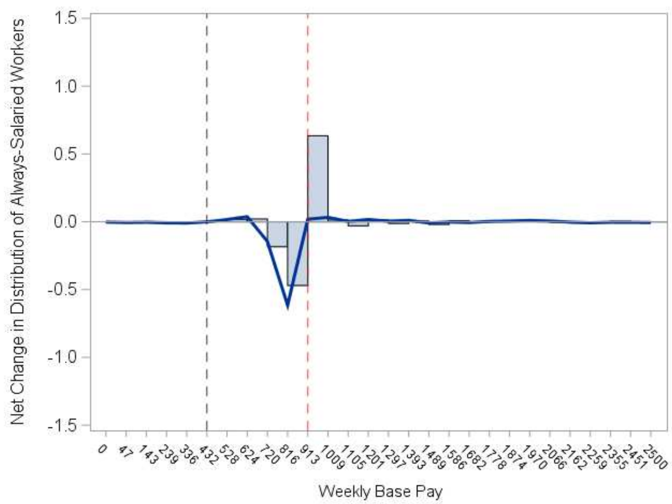
Figure 8: Decomposing the Effect of Raising the OT Exemption Threshold on the Income Distribution of Salaried Workers



(a) Effect on Net Employment



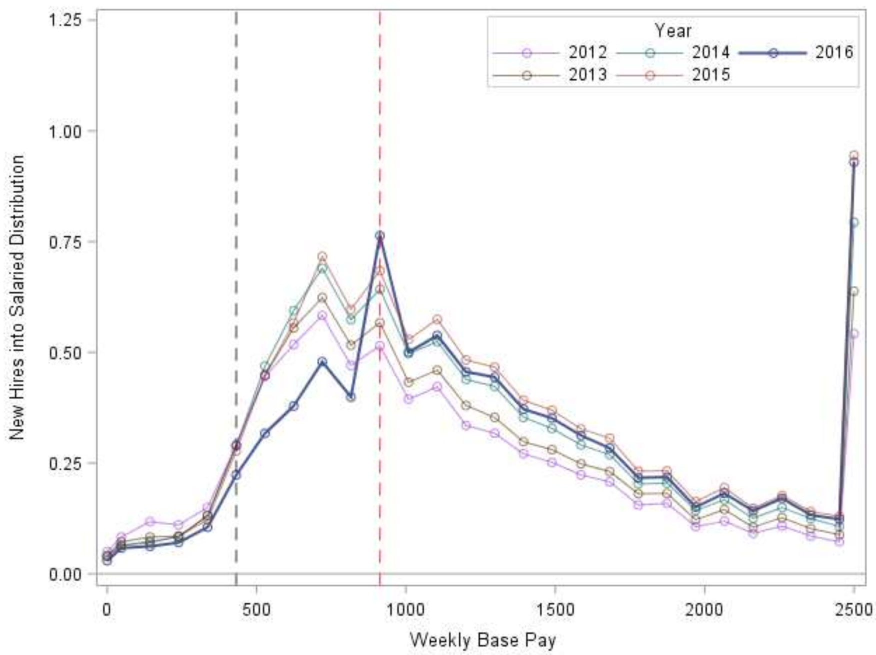
(b) Effect on Net Reclassifications



(c) Effect on Always-Salaried Workers

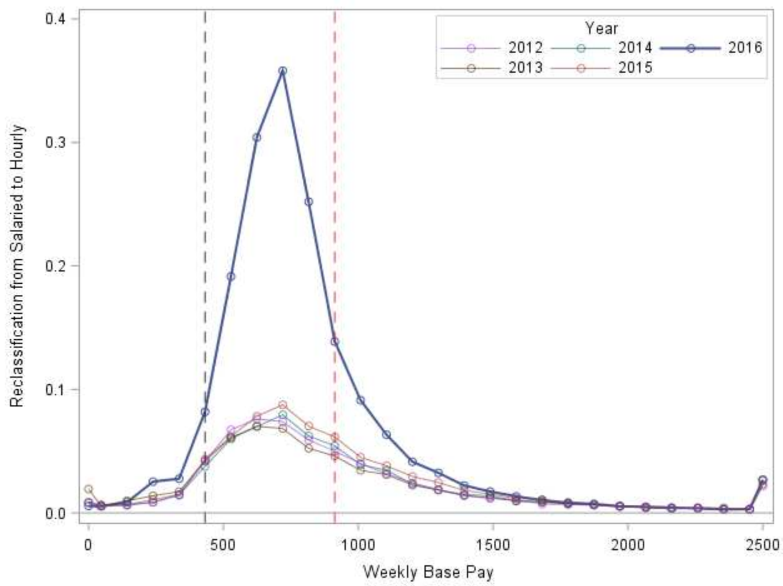
Notes: Each graph in this figure shows the average effect of the policy on the flow of workers into each bin of the salaried distribution (the blue bars) and the cumulative sum of these effects (the blue line). Figure (a) plots the effect of the policy on the net flow of workers from unemployment. Figure (b) plots the effect on the number of salaried workers in each bin due to reclassifications (in blue) and the effect on the number of hourly workers in each bin due to reclassifications (in red). Figure (c) plots the effect on the net flow of workers within the salary distribution. The vertical black and red lines are at \$432 and \$913, respectively.

Figure 9: Distribution of New Hires Between April and December, Salaried Workers

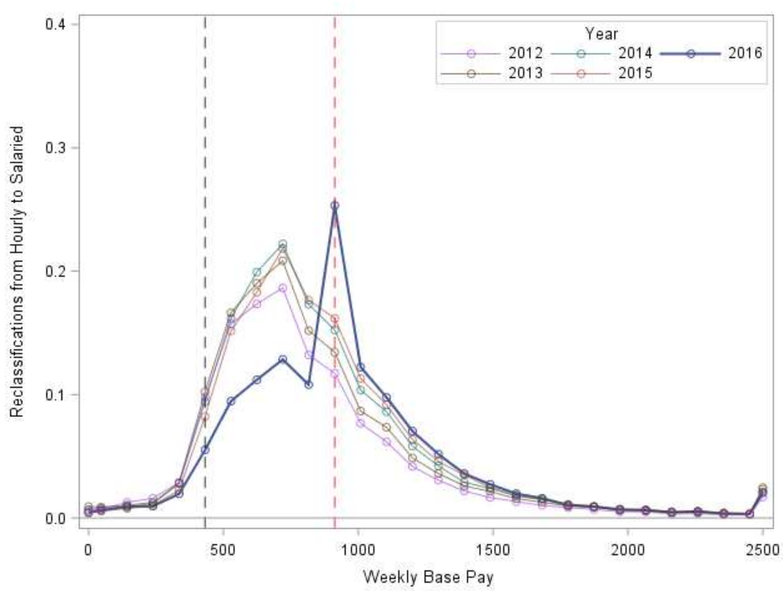


Notes: The figure shows the frequency distribution of base pays in December of each year between 2012 and 2016, averaged across firms, for newly hired salaried workers between April and December.

Figure 10: Distribution of Reclassifications Flows In and Out of the Salaried Distribution



(a) Reclassifications from Salaried to Hourly



(b) Reclassifications from Hourly to Salaried

Notes: Figure (a) shows the frequency distribution of base pays in April of each year between 2012 and 2016, averaged across firms, for workers who are salaried in April, hourly in December, and employed in the same firm in both months. Figure (b) shows the frequency distribution of base pays in December of each year between 2012 and 2016, averaged across firms, for workers who are hourly in April, salaried in December, and employed in the same firm in both months.

Table 1: Effect of Raising the OT Exemption Threshold on Employment and Income

	(1) Salaried	(2) Hourly	(3) Pooled	(4) Stayers
Number of Jobs				
[0,432)	-0.043	-0.950^{***}	-0.992^{***}	-0.030
	(0.045)	(0.232)	(0.238)	(0.24)
[432,913)	-2.481^{***}	1.686^{***}	-0.795^{***}	-0.626^{**}
	(0.085)	(0.251)	(0.260)	(0.245)
[913,1009)	0.739^{***}	0.21^{***}	0.949^{***}	0.651^{***}
	(0.030)	(0.041)	(0.053)	(0.111)
[1009,1778)	-0.171^{**}	0.095	-0.076	-0.011
	(0.075)	(0.097)	(0.136)	(0.314)
Wage Bill				
[0,432)	65.76	-263.26^{***}	-197.49^{**}	-8.92
	(10.36)	(82.32)	(83.25)	()
[432,913)	-1811.33^{***}	1823.32^{***}	-691.96^{***}	-490.15^{***}
	(61.80)	(159.21)	(167.03)	(18.81)
[913,1009)	686.43^{***}	197.48^{***}	800.91^{***}	611.64^{***}
	(28.60)	(40.26)	(51.16)	(17.45)
[1009,1778)	-185.44^{*}	127.43	-58.01	1.31
	(104.75)	(129.68)	(181.23)	(22.80)
Cumulative Jobs Effect	-1.955^{***}	1.041^{**}	-0.914^{*}	-0.02
	(0.141)	(0.417)	(0.472)	(0.478)
Δ Jobs per Aff. Workers	-0.148^{***}	0.079^{**}	-0.069^{*}	0.002
	(0.011)	(0.032)	(0.036)	(0.036)
Cumulative Pay Effect	-1247.58^{***}	1181.02^{***}	-66.55	113.89^{***}
	(140.54)	(282.02)	(353.25)	(17.45)
Δ Pay per Aff. Workers	-94.59^{***}	89.54^{***}	5.05	8.63^{***}
	(10.66)	(21.38)	(26.78)	(1.32)
Δ Pay per Job Lost			73.13	
			()	
%Δ Average Base Pay				0.014^{***}
				(0.002)
Affected Workers	13.19	13.19	13.19	13.19
No. Jobs in [455,913)	13.19	65.76	78.95	10.84
Wage Bill in [455,913)	\$9,594.77	\$42,130	\$51,724.77	\$7,923.89
Total No. Jobs	69.18	133.13	202.64	59.82
Number of Firms	58,584	58,584	58,584	58,584

Notes: First four rows of column (1) report the effect of the raising the OT threshold on the number of salaried workers in each interval of base pay in December 2016, where the coefficients are sums of the bin-specific effects estimated from eq. 3. The next four rows report the effect on the total base pay paid to workers in each interval of the salary distribution. Columns 2 reports the analogous effects on the hourly distribution. Column (3) reports the effect of the policy on the pooled sample, computed as the sum of columns (1) and (2). Column (4) reports the effect of the policy on the frequency distribution of workers who are paid by salary in April 2016 and remain employed (as salaried or hourly) in December 2016.

The cumulative jobs effect is the sum of rows (1) to (4). The cumulative pay effect equals the sum of rows (5) to (8). "Δ Jobs per Aff. Workers" and "Δ Pay per Aff. Workers" equal the cumulative jobs and cumulative pay effects divided by the number of affected workers, respectively. "Affected workers" is the number of salaried workers in the average firm with base pay between \$455 and \$913 in April 2016, whereas "No. Jobs in [455,913)" is the number of workers in the subsample that satisfy those conditions. "Δ Pay per Job Lost" equals the cumulative pay effect divided by the cumulative jobs effect. "%Δ Average Base Pay" equals the cumulative pay effect divided by "Wage Bill in [455,913)", which I define as the wage bill paid to the set of workers in "No. Jobs in [455,913)". The second last row is the number of workers within the subsample in the average firm in April 2016. The last row is the number of firms in the balanced panel of April and December 2016. Due to computational difficulties, standard errors on the job effects in column (4) are unclustered, and the standard errors for the wage effects in column (4) are clustered by firm assuming a constant correlation between firms. All other robust standard errors in parentheses are clustered by firm, assuming independence across firms. $^{*}p < .1, ^{**}p < .05, ^{***}p < .01$

Table 2: Effect of Raising the OT Exemption Threshold on the Net Flow of Workers Into the Salaried and Hourly Distributions

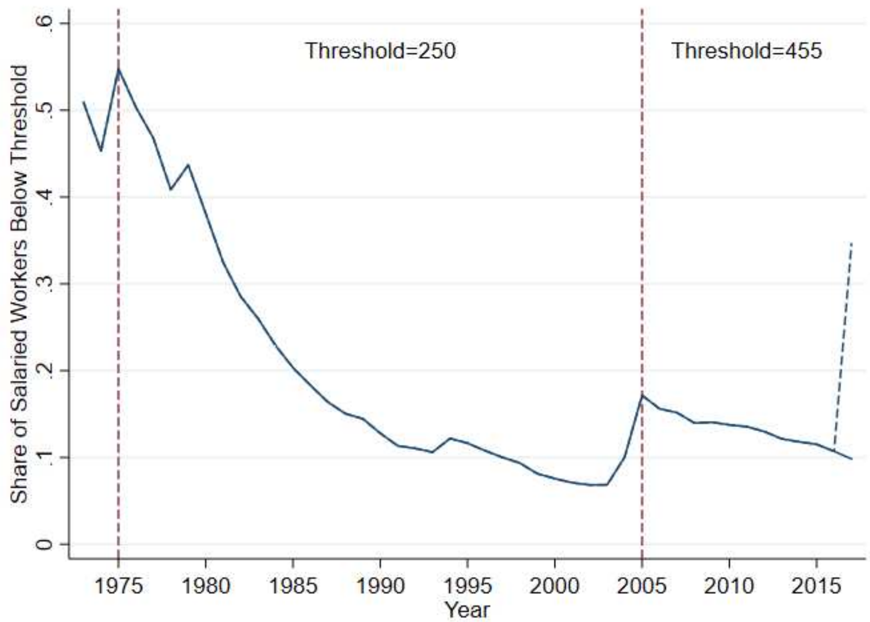
	Salaried Distribution		Reclassifications		
	(1)	(2)	(3)	(4)	(5)
	Employment	Within Dist.	Salaried	Hourly	Sum
Number of Jobs					
[0,432)	-0.054**	-0.009	-0.025	0.066***	0.041**
	(0.027)	(0.027)	(0.017)	(0.01)	(0.017)
[432,913)	−0.562***	−0.606***	−1.212***	1.016***	−0.195***
	(0.039)	(0.028)	(0.062)	(0.059)	(0.022)
[913,1009)	0.154***	0.634***	0.016	0.099***	0.115***
	(0.012)	(0.013)	(0.013)	(0.012)	(0.009)
[1009,1778)	0.041	-0.024	−0.101***	0.143	0.042***
	(0.051)	(0.036)	(0.036)	(0.036)	(0.008)
Wage Bill					
[0,432)	−117.96***	-1.47	-10.53	24.24***	13.72**
	(4.85)	(2.29)	(5.38)	(2.91)	(5.53)
[432,913)	−488.26***	−475.48***	−875.08***	717.28***	−157.80***
	(33.37)	(19.63)	(43.91)	(39.71)	(13.49)
[913,1009)	147.28***	596.60***	12.17	94.63***	106.80***
	(13.75)	(20.66)	(12.31)	(11.15)	(8.57)
[1009,1778)	56.273	-12.622	−115.87***	171.94***	56.07***
	(88.56)	(24.34)	(45.43)	(47.36)	(10.66)
Cumulative Jobs Effect	−0.425***	-0.005	−1.321***	1.324***	0.003
	(0.082)	(0.054)	(0.082)	(0.081)	(0.009)
Δ Jobs per Aff. Workers	−0.032***	0.000	−0.100***	0.100***	0.000
	(0.006)	(0.004)	(0.006)	(0.006)	(0.001)
Cumulative Pay Effect	−402.67***	107.13***	−989.31***	1008.09***	18.79*
	(112.98)	(19.89)	(73.59)	(71.21)	(10.48)
Δ Pay per Aff. Workers	-30.53***	8.12***	-74.97***	76.42***	1.42*
	(8.57)	(1.51)	(5.58)	(5.55)	(0.79)
%Δ Average Base Pay		0.015***			0.014*
		(0.003)			(0.008)
Affected Workers	13.19	13.19	13.19	13.19	13.19
No. Jobs in [455,913)	2.34	9.66	1.18	0.73	1.91
Wage Bill in [455,913)	$1,664.43	$7,075.10	$848.79	$494.90	$1,354.56
Total No. Jobs	9.36	58.03	1.79	1.31	3.10
Number of Firms	58,584	58,584	58,584	58,584	58,584

Notes: The first four rows of columns (1)-(3) report the effect of raising the OT threshold on the net flow of workers into each interval of the salaried distribution from unemployment, from within the salaried distribution, and from the hourly distribution between April and December 2016. The next four rows show the change in the total base pay in each interval due to the above flow effects. Column (4) reports analogous estimates on the effect of the policy on the flow of workers into the hourly distribution from the salaried distribution. All the flow effects are sums of estimates from eq. **??**. Column (5) reports the effect of the policy on the pooled frequency distribution due to reclassifications, computed as the sum of columns (3) and (7).

The cumulative jobs effect is the sum of rows (1) to (4). The cumulative pay effect equals the sum of rows (5) to (8). "Δ Jobs per Aff. Workers" and "Δ Pay per Aff. Workers" equal the cumulative jobs and cumulative pay effects divided by the number of affected workers, respectively. "Affected workers" is the number of salaried workers in the average firm with base pay between $455 and $913 in April 2016, whereas "No. Jobs in [455,913)" is the number of workers in the subsample that satisfy those conditions. "%Δ Average Base Pay" equals the cumulative pay effect divided by "Wage Bill in [455,913)", which I define as the wage bill paid to the set of workers in "No. Jobs in [455,913)". The second last row is the number of workers within the subsample in the average firm in April 2016. The last row is the number of firms in the balanced panel of April and December 2016. Robust standard errors in parentheses are clustered by firm. $^{*}p < .1,^{**} p < .05,^{***} p < .01$

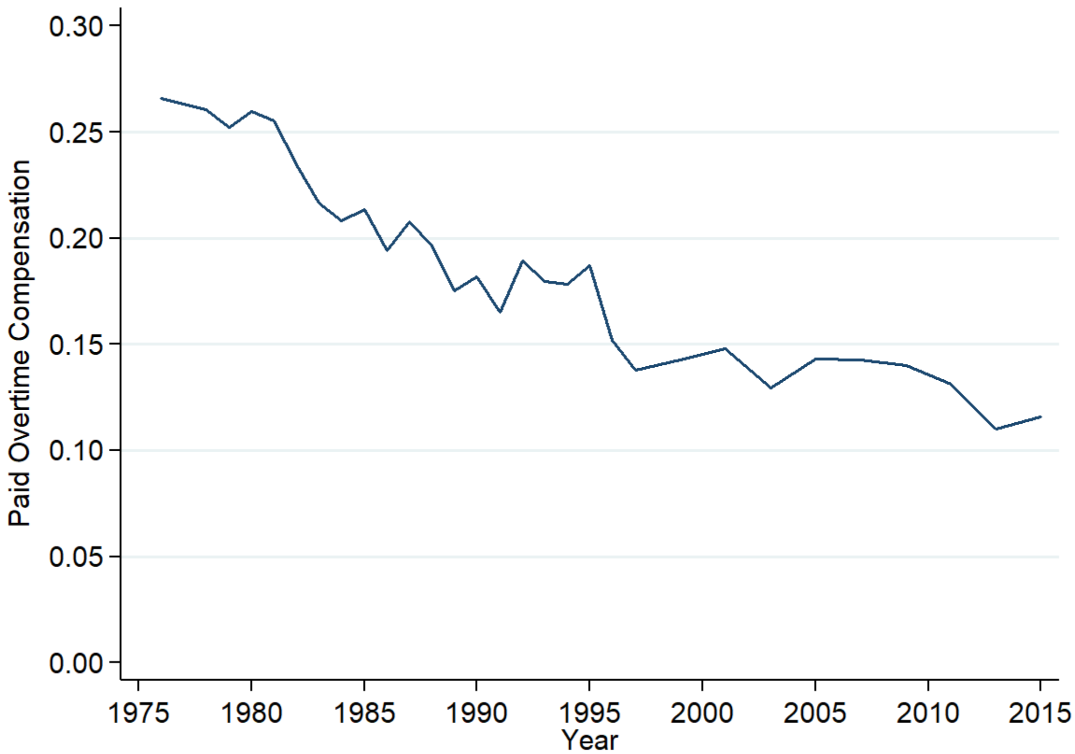
Appendix A. Additional figures and tables

Appendix Figure A.1: Percent of Salaried Workers Below the FLSA OT Exemption Threshold



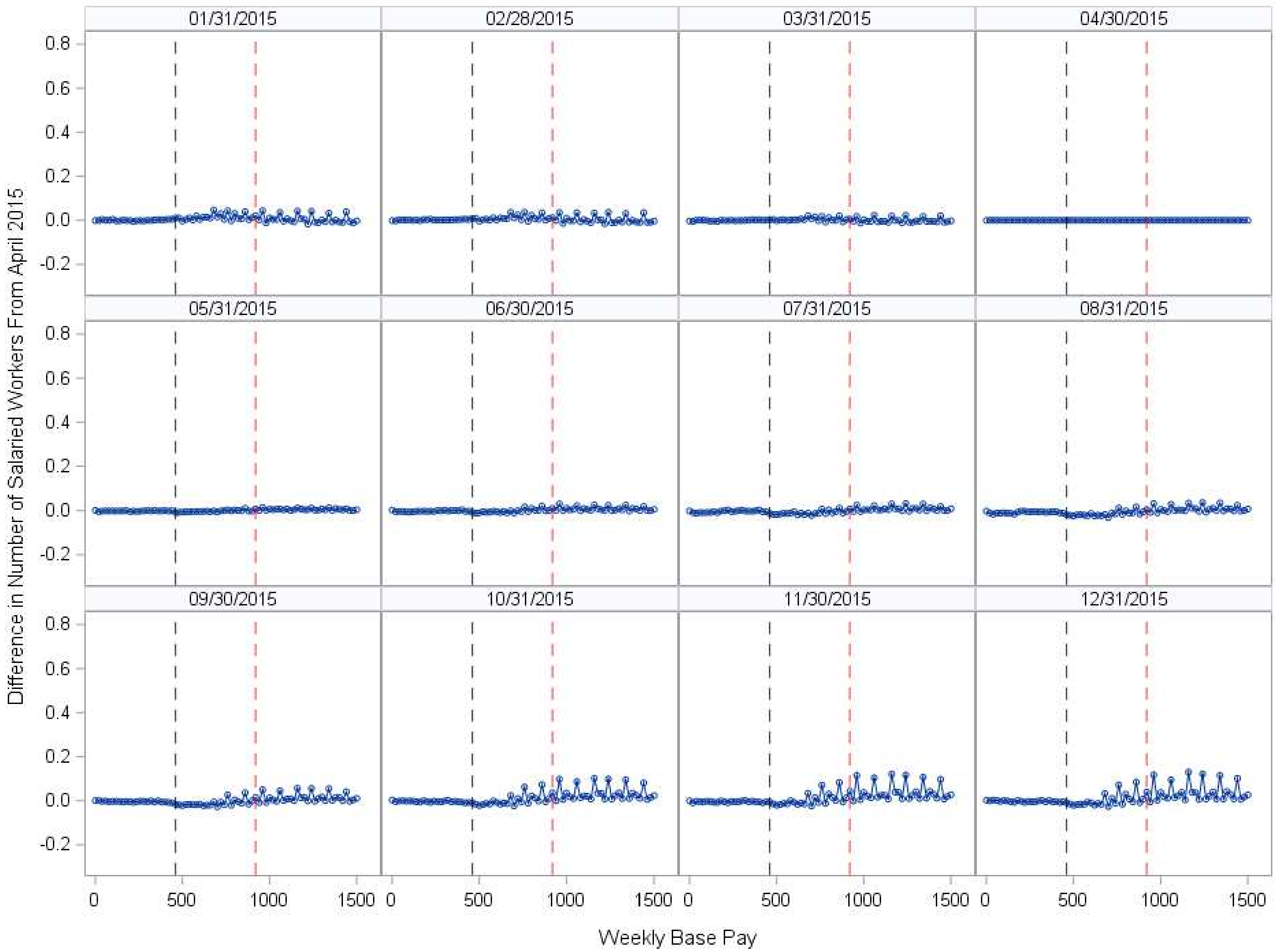
Notes: The figure shows the share of all salaried workers in the May extracts of the CPS who report usual weekly earnings below the effective FLSA overtime exemption threshold from 1973 to 2017. The threshold increased from \$200 per week to \$250 per week in January 1975, and then to \$455 in August 2004. The dotted blue line shows the percent of salaried workers with usual weekly earnings below the \$913 per week threshold announced in the 2016 policy.

Appendix Figure A.2: Percent of Salaried Workers Eligible for Overtime



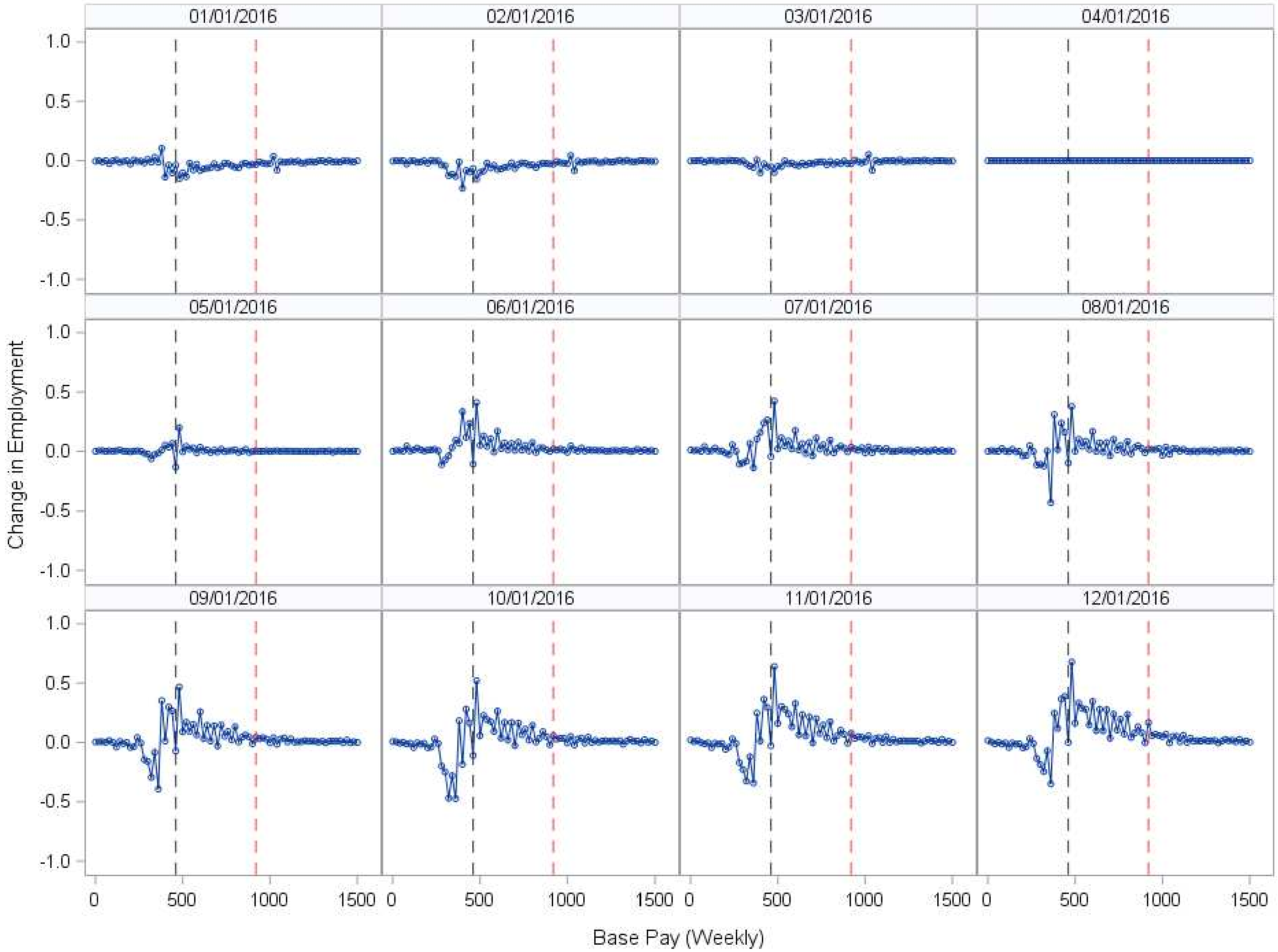
Notes: This figure shows the percent of salaried workers in the PSID who respond yes to the question "If you were to work more hours than usual during some week, would you get paid for those extra hours of work".

Appendix Figure A.3: Frequency Distribution of Salaried Workers by Month, Differenced by the Distribution in April 2015



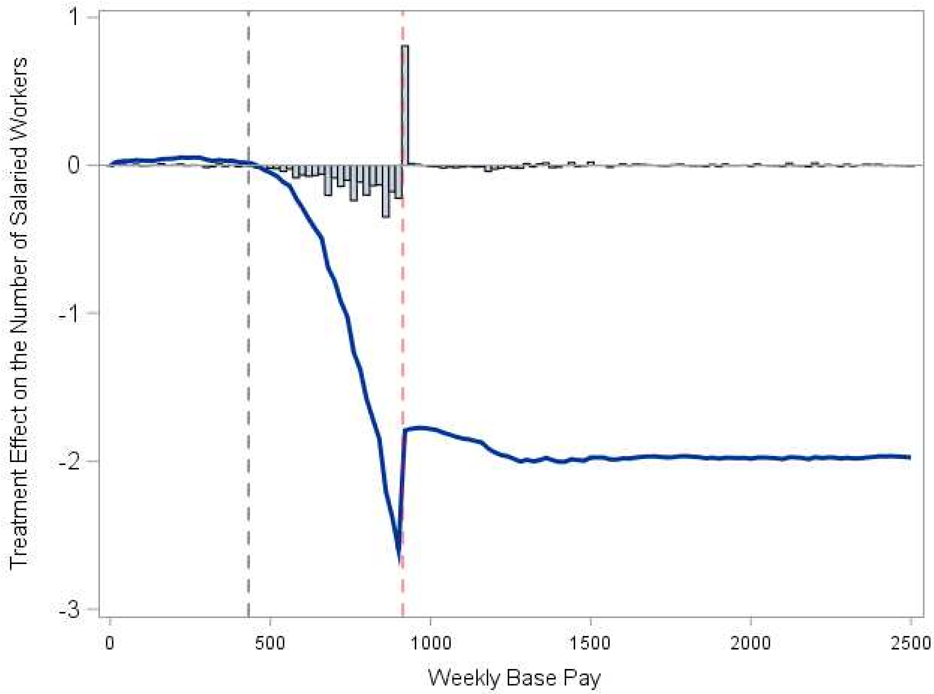
Notes: The figure shows the frequency distribution of weekly base pays in each month of 2015, subtracted by the frequency distribution in April 2015. For each month, I scale the distribution by the number of firms in the superset of all firms that appear in the data that month and in April 2015. Within each graph, the bins are $20 wide except for the first bin which goes from $0 to $12.99. The vertical black dashed line is at the bin containing $455, and the red dashed line is at the bin containing $913.

Appendix Figure A.4: Frequency Distribution of Hourly Workers by Month, Differenced by the Distribution in April 2016



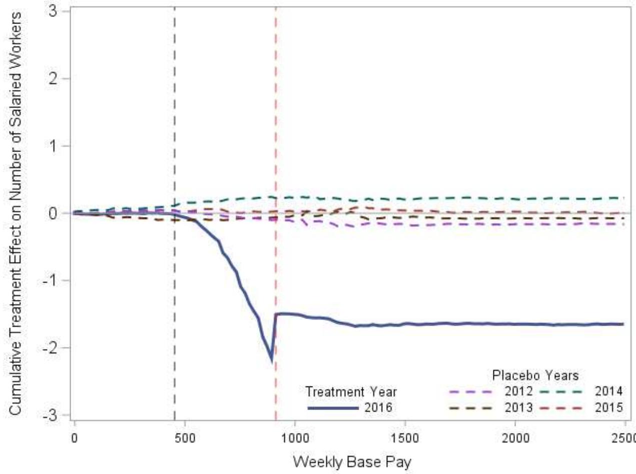
Notes: The figure shows the frequency distribution of weekly base pays of hourly workers in each month of 2016 and 2017, subtracted by the frequency distribution in April 2016. For each month, I scale the distribution by the number of firms in the superset of all firms that appear in the data that month and in April 2016. Within each graph, the bins are \$20 wide except for the first bin which goes from \$0 to \$12.99. The vertical black dashed line is at the bin containing the overtime exemption threshold in April (\$455), while the red dashed line is at the bin containing the threshold (\$913) that was supposed to go into effect on December 1, 2016.

Appendix Figure A.5: Effect of Raising the OT Threshold on the Number of Salaried Workers, $20 Bins

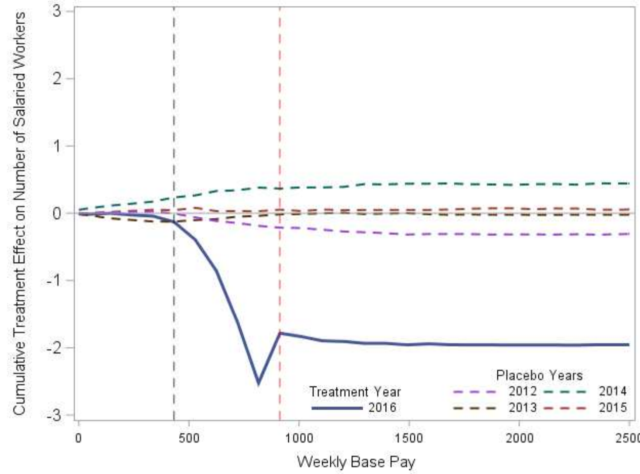


Notes: This figure shows the effect of the policy on the number of salaried workers in each $20 bin in December 2016. The treatment effects are estiamted using equation 3. The solid blue line is the running sum of these effects. The vertical black and red lines are at the old and new OT exemption thresholds ($455 and $913), respectively.

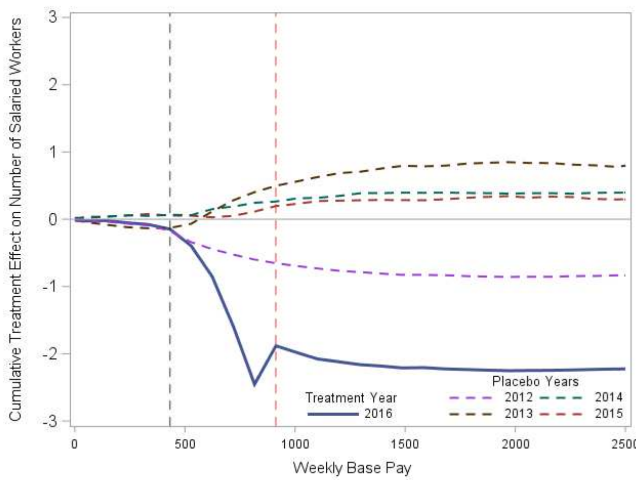
Appendix Figure A.6: Robustness of the Estimated Effects on the Frequency Distribution of Salaried Workers



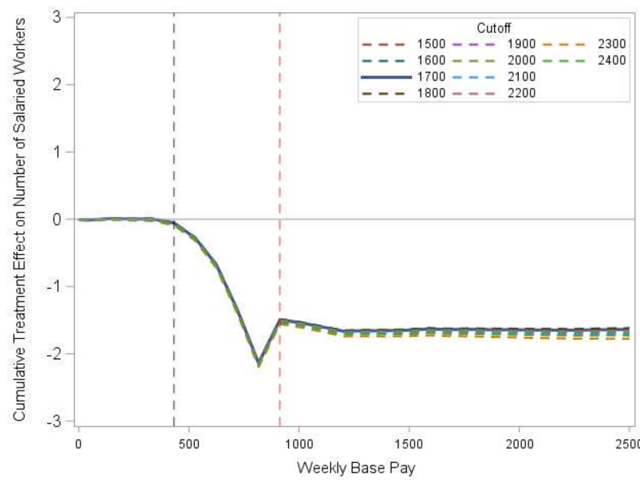
(a) Robust to Bin Width of $20



(b) Robust to Relaxing γ's



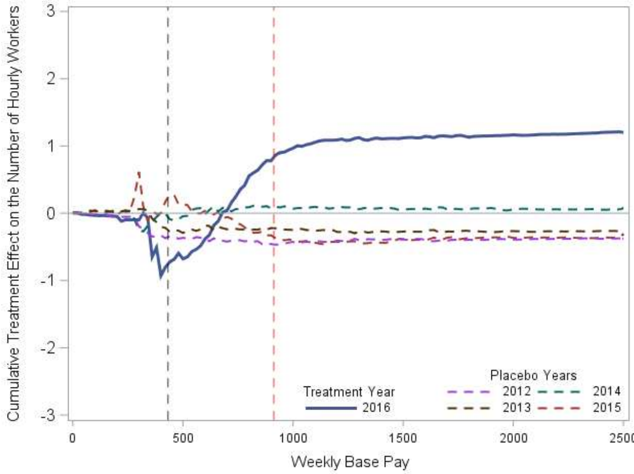
(c) Robust to Unbalanced Panel



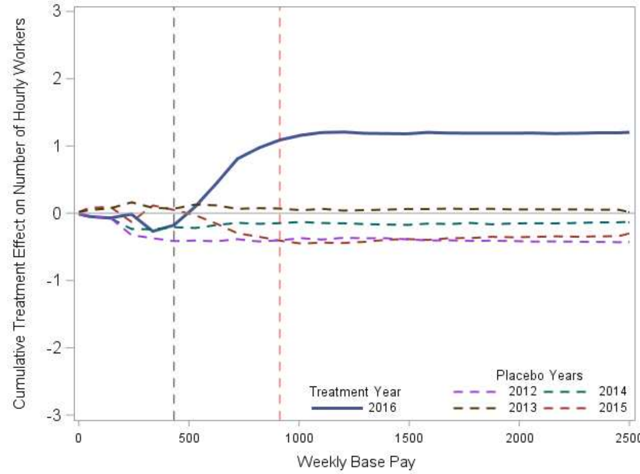
(d) Robust to Cutoff

Notes: Each figure presents the cumulative effect of raising the OT threshold in 2016 on the number of salaried workers up to that bin, using a different sample or specification for each figure. Figure (a) estimates the bin-by-bin treatment effects for each year between 2012 and 2016 from equation 3 using a bin width of $20 per bin. The lines in figures (b), (c), and (d) were estimated using bins of width $96.15. Figure (b) estimates the treatment effects without restricting $\gamma_1 = 1$ and γ_0=0 for bins smaller than $675. Figure (c) uses an unbalanced panel whereby if a firm is missing in one month, its employment is coded as 0 in every bin. Each line in figure (d) is estimated using a different cutoff j^*, where I use all bins $j > j^*$ to estimate the γ_1 and γ_0 in equation 3.

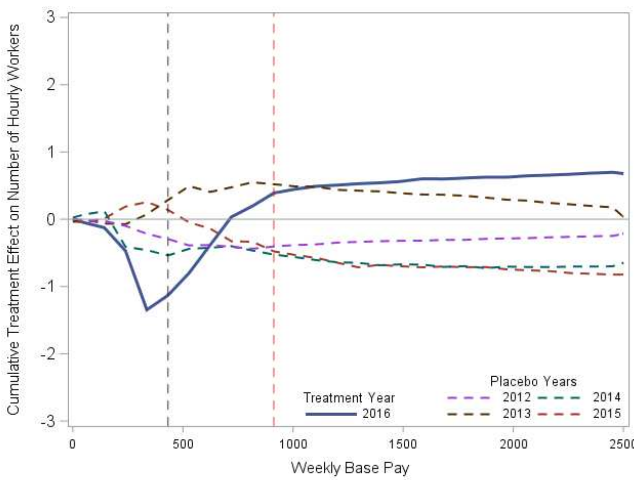
Appendix Figure A.7: Placebo Tests of the Cumulative Effects on the Frequency Distribution of Hourly Workers



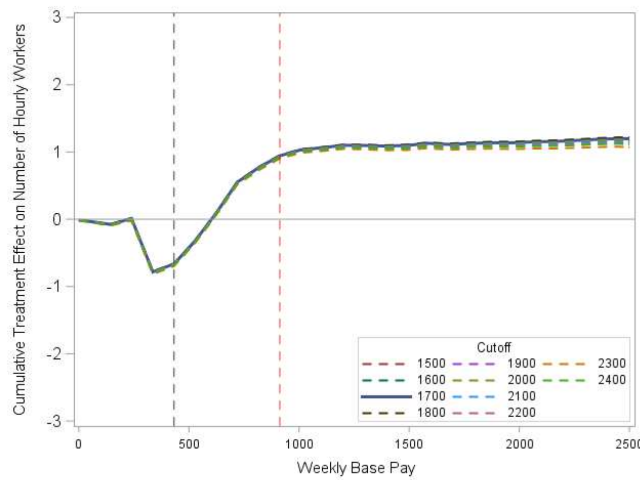
(a) Robust to Bin Width of $20



(b) Robust to Dropping MW States



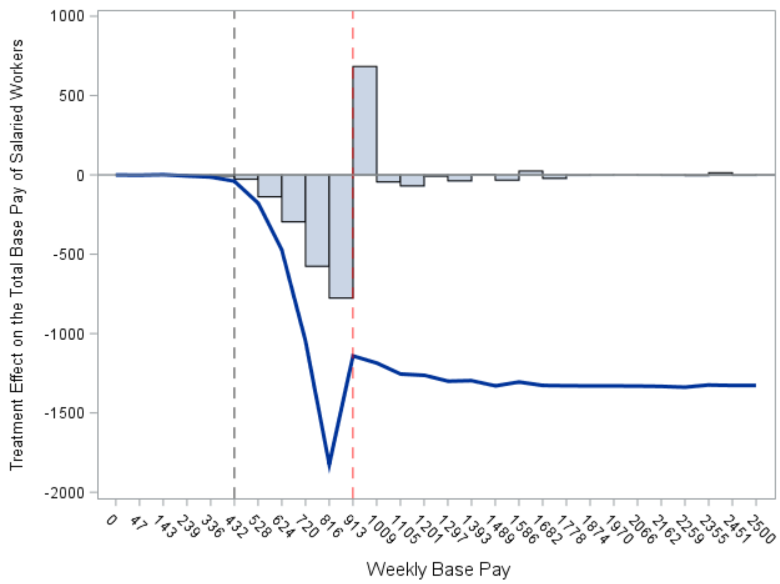
(c) Robust to Unbalanced Panel



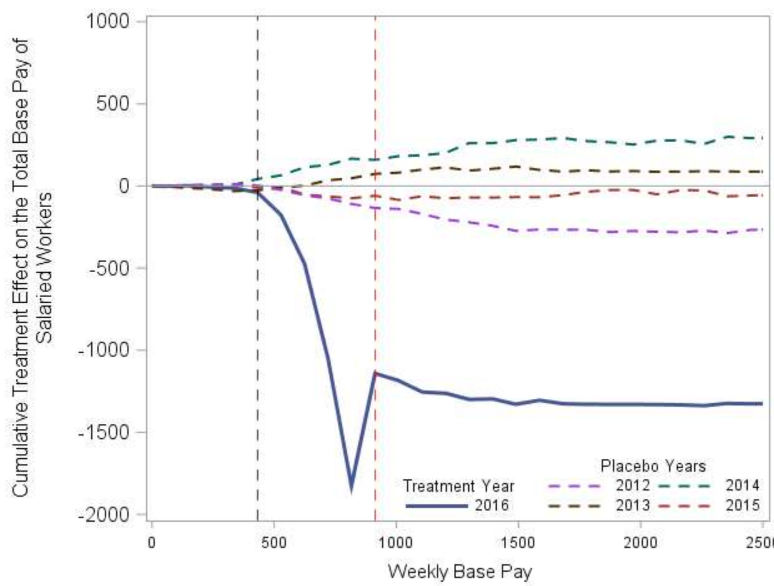
(d) Robust to Cutoff

Notes: Each figure presents the cumulative effect of raising the OT threshold in 2016 on the number of hourly workers up to that bin, using a different sample or specification for each figure. Figure (a) estimates the bin-by-bin treatment effects for each year between 2012 and 2016 from equation 3 using a bin width of $20 per bin. The lines in figures (b), (c), and (d) were estimated using bins of width $96.15. Figure (b) estimates the treatment effects after dropping the 27 states with a state-specific minimum wage. Figure (c) uses an unbalanced panel whereby if a firm is missing in one month, its employment is coded as 0 in every bin. Each line in figure (d) is estimated using a different cutoff j^*, where I use all bins $j > j^*$ to estimate the γ_1 and γ_0 in equation 3.

Appendix Figure A.8: Effect of Raising the OT Exemption Threshold on the Total Base Pay within Each Bin of Salaried Workers' Weekly Base Pay



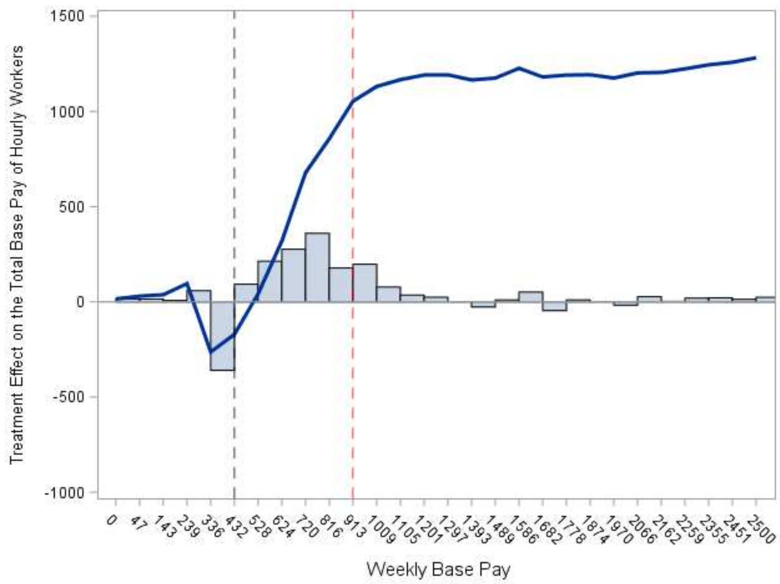
(a) Estimates of the Effect on Total Base Pay of Salaried Workers



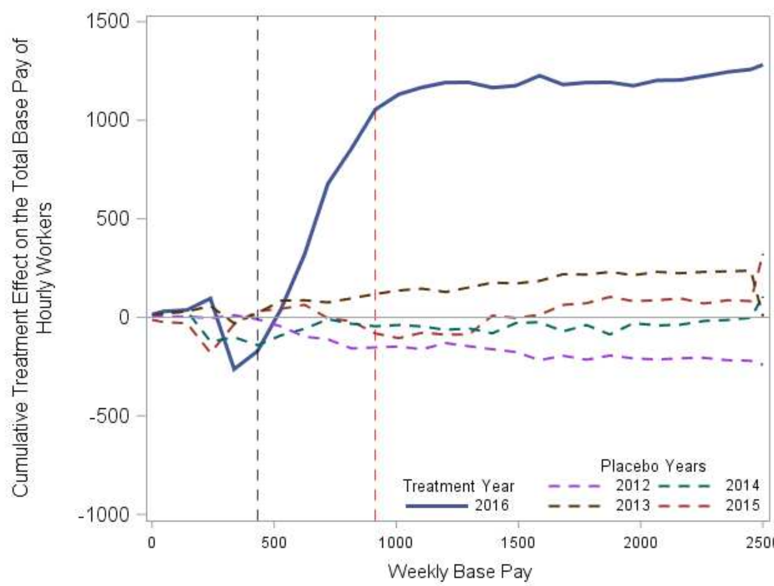
(b) Placebo Test of Effect on Total Base Pay

Notes: Each bar in panel (a) shows the effect of the policy on the total base pay paid to salaried workers in that $96.15 bin in Dec 2016. The treatment effects are estimated using equation 3, where the outcome variable is total base pay. The solid blue line is the running sum of these effects. Panel (b) shows the cumulative effect of raising the OT exemption threshold on the total base pay paid to salaried workers in December of each year between 2012 and 2016. The solid blue line in panel (b) is the same as the one in panel (a). The dotted graphs are similarly defined running sums, except the effect of the policy is estimated using the December and April distributions of the labeled year and the preceding adjacent year. In both graphs, the vertical black and red lines are at the start of the bins that contain the old and new OT exemption thresholds ($455 and $913), respectively.

Appendix Figure A.9: Effect of Raising the OT Exemption Threshold on the Total Base Pay within Each Bin of Hourly Workers' Weekly Base Pay



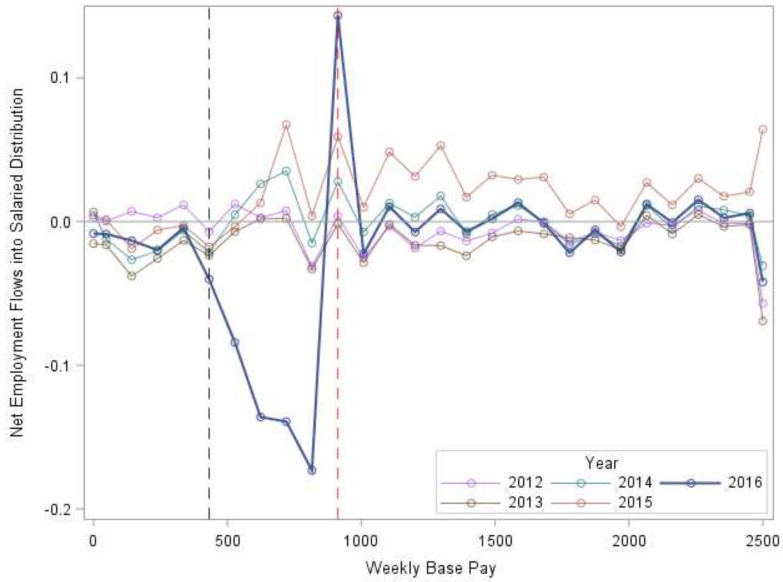
(a) Estimates of the Effect on Total Base Pay of Hourly Workers



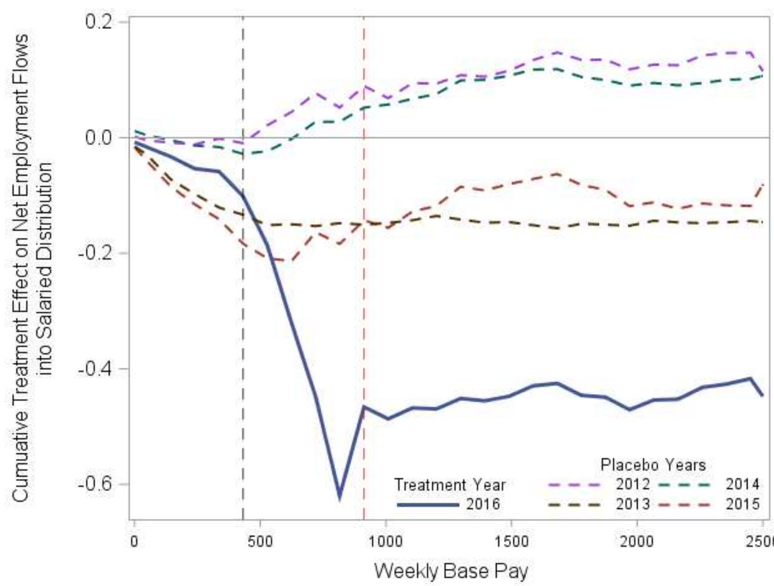
(b) Placebo Test of Effect on Total Base Pay

Notes: Each bar in panel (a) shows the effect of the policy on the total base pay paid to hourly workers in that \$96.15 bin in Dec 2016. The treatment effects are estimated using equation 3, where the outcome variable is total base pay. The solid blue line is the running sum of these effects. Panel (b) shows the cumulative effect of raising the OT exemption threshold on the total base pay paid to hourly workers in December of each year between 2012 and 2016. The solid blue line in panel (b) is the same as the one in panel (a). The dotted graphs are similarly defined running sums, except the effect of the policy is estimated using the December and April distributions of the labeled year and the preceding adjacent year. In both graphs, the vertical black and red lines are at the start of the bins that contain the old and new OT exemption thresholds (\$455 and \$913), respectively.

Appendix Figure A.10: Analysis of the Employment Flows into the Salaried Distribution



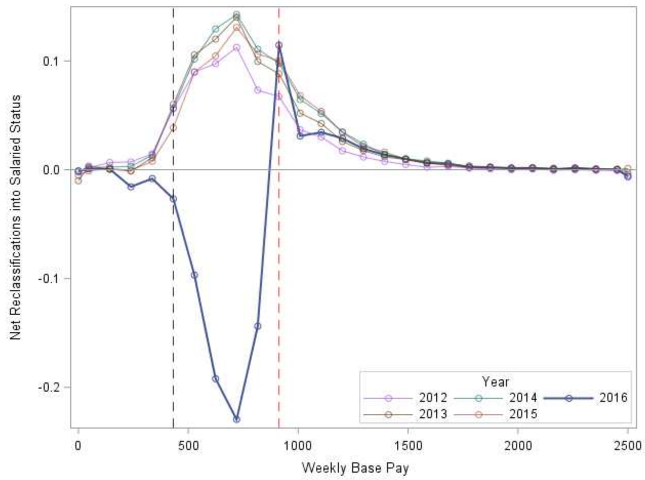
(a) Distribution of Net Employment by Year



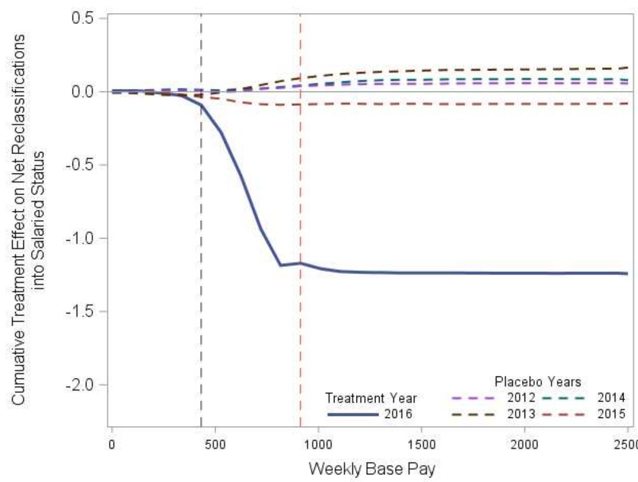
(b) Placebo Test of the Net Employment Effect

Notes: Figure (a) shows the frequency distribution of net flows into the salaried distribution from unemployment, between April and December of each year. For each year, the corresponding graph is equal to the difference between the distribution of new hires and the the distribution of separations. Figure (b) estimates the cumulative effect of the OT exemption threshold policy on the net employment flows of each year between 2012 and 2016. The effect at each bin is estimated via equation **??** and the lines are the sum of the running sum of the effects across bins.

Appendix Figure A.11: Analysis of Reclassifications in and out of the Salaried Distribution



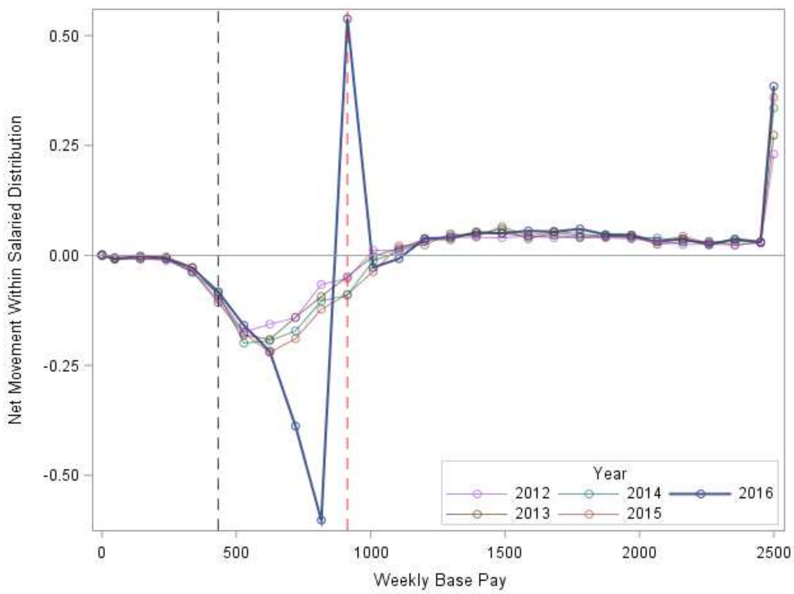
(a) Distribution of Net Reclassifications by Year



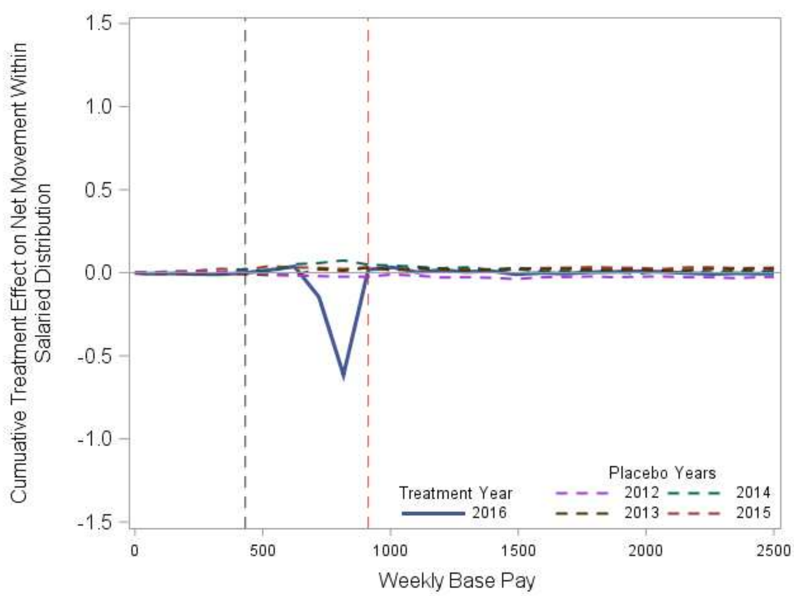
(b) Placebo Test of Net Reclassification Effects

Notes: Figure (a) shows the distribution of net flows into the salaried distribution from the hourly distribution, between April and December of each year. For each year, the corresponding graph is equal to the difference between the distribution of salaried workers in April who become hourly in December, and the distribution of salaried workers in December who were hourly in April. Figure (b) estimates the cumulative effect of the OT exemption threshold policy on the net reclassification flows of each year between 2012 and 2016. The effect at each bin is estimated via equation **??** and the lines are the sum of the running sum of the effects across bins.

Appendix Figure A.12: Analysis of Flows Within the Salaried Distribution Between April and December



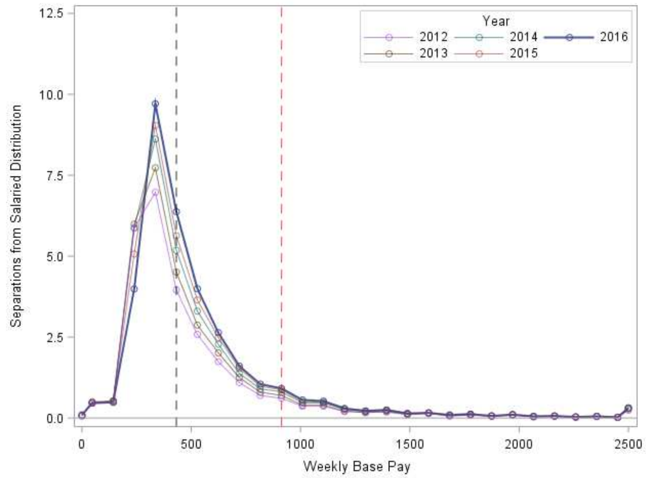
(a) Distribution of Flows Within Salaried Distribution



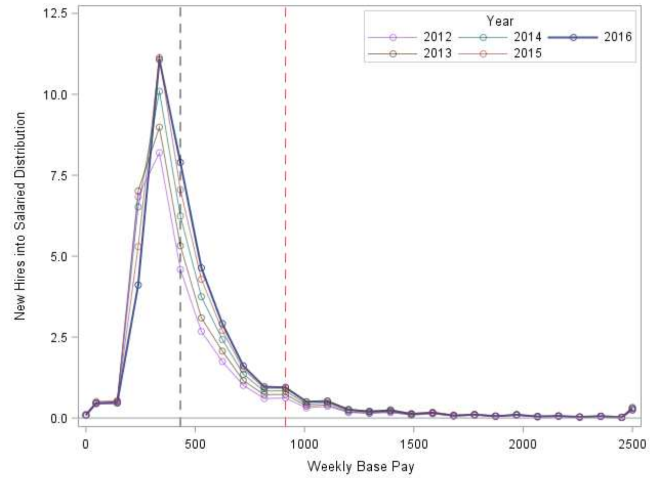
(b) Placebo Test of the Effect Within Salaried Distribution

Notes: Figure (a) shows the net flow into each bin of the salaried distribution from all other bins in the salaried distribution, between April and December of each year. Figure (b) estimates the cumulative effect of the OT exemption threshold policy on the net flow of workers within the salaried distribution of each year between 2012 and 2016. The effect at each bin is estimated via equation **??** and the lines are the sum of the running sum of the effects across bins.

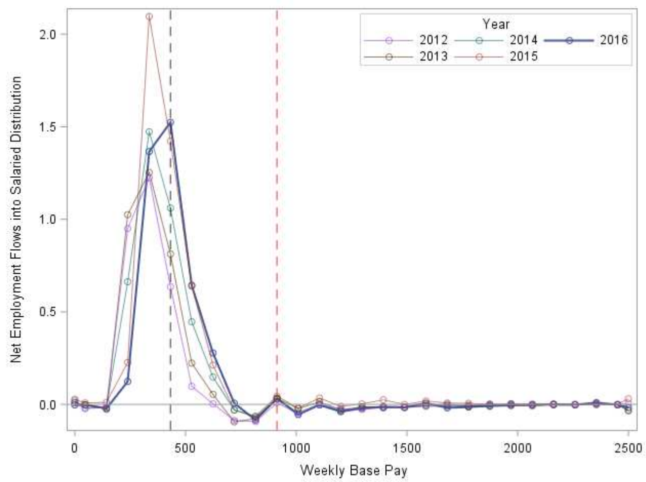
Appendix Figure A.13: Analysis of the Employment Flows into the Hourly Distribution



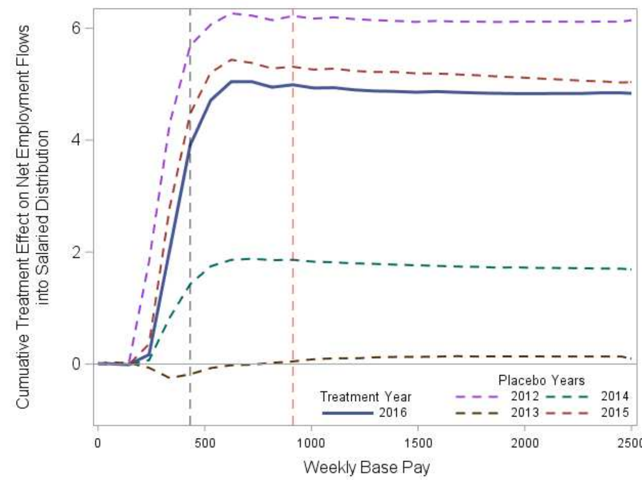
(a) Separations Between April and December



(b) New Hires Between April and December



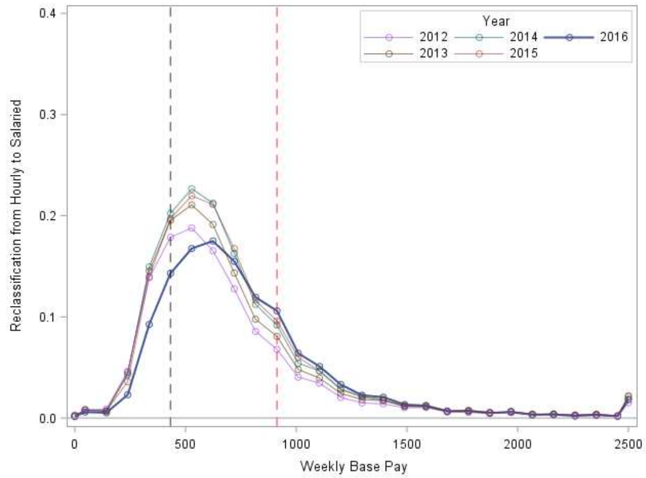
(c) Distribution of Net Employment by Year



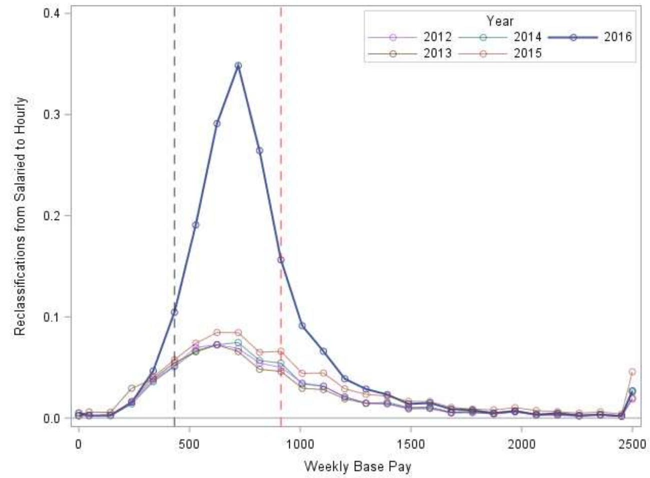
(d) Placebo Test of the Net Employment Effect

Notes: Figure (a) shows the distribution of separations from the hourly distribution between April and December of each year. Figure (b) shows the distribution of new hires into the hourly distribution between April and December of each year. Figure (c) shows the distribution of net flows into the hourly distribution from unemployment, between April and December of each year. It is equal to the difference between figures (b) and (a). Figure (d) estimates the cumulative effect of the OT exemption threshold policy on the net employment flows of each year between 2012 and 2016. The effect at each bin is estimated via equation **??** and the lines are the sum of the running sum of the effects across bins.

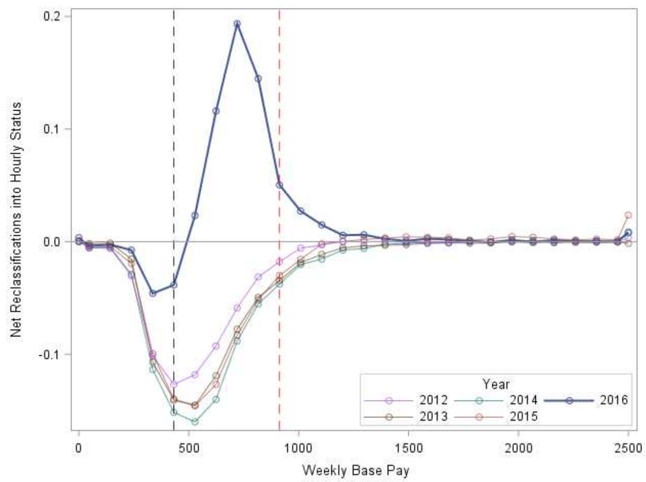
Appendix Figure A.14: Analysis of Reclassifications in and out of the Hourly Distribution



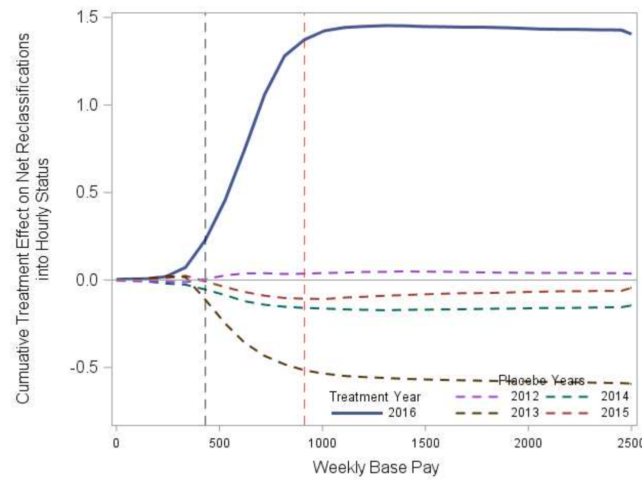
(a) Reclassifications from Hourly to Salaried



(b) Reclassifications from Salaried to Hourly



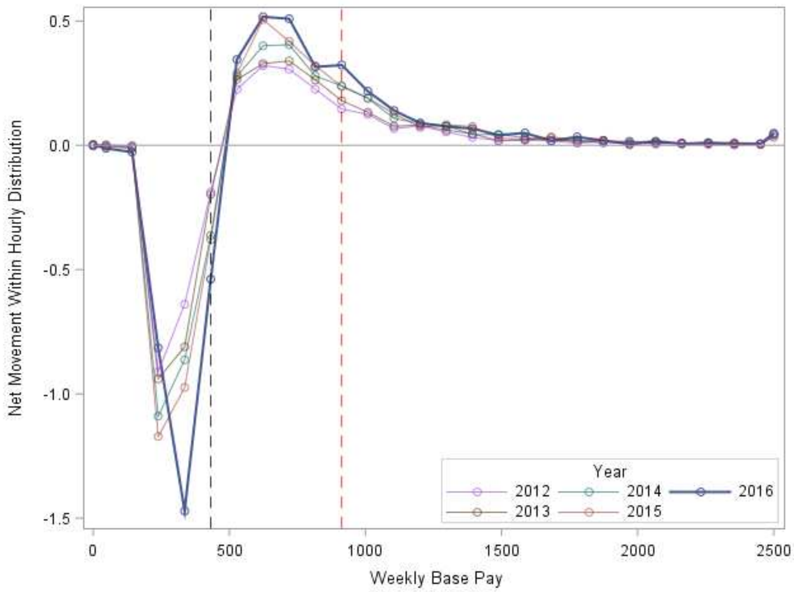
(c) Distribution of Net Reclassifications by Year



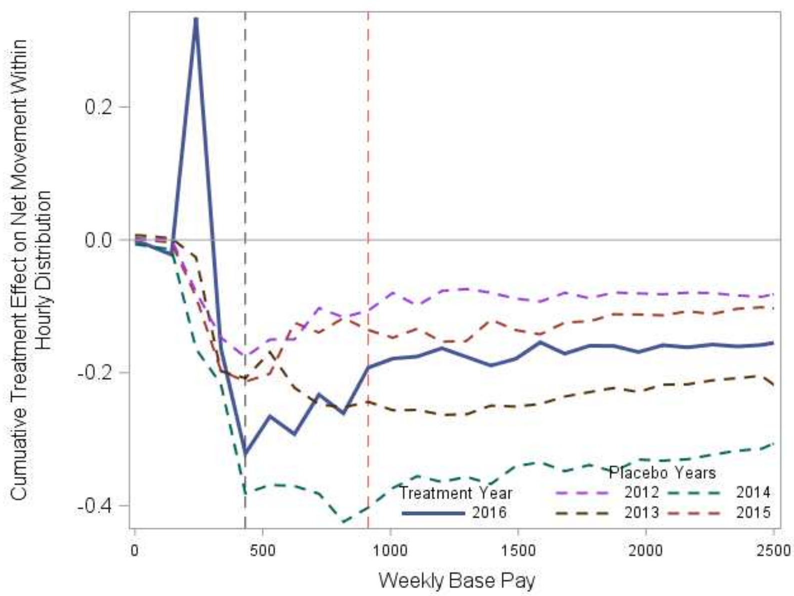
(d) Placebo Test of Net Reclassification Effects

Notes: Figure (d) shows the distribution of reclassifications out of the hourly distribution between April and December of each year. Figure (b) shows the distribution of reclassifications into the hourly distribution between April and December of each year. Figure (c) shows the distribution of net flows into the hourly distribution from the salaried distribution, between April and December of each year. It is equal to the difference between figures (b) and (a). Figure (d) estimates the cumulative effect of the OT exemption threshold policy on the net reclassification flows of each year between 2012 and 2016. The effect at each bin is estimated via equation **??** and the lines are the sum of the running sum of the effects across bins.

Appendix Figure A.15: Analysis of Flows Within the Hourly Distribution Between April and December



(a) Distribution of Flows Within Hourly Distribution



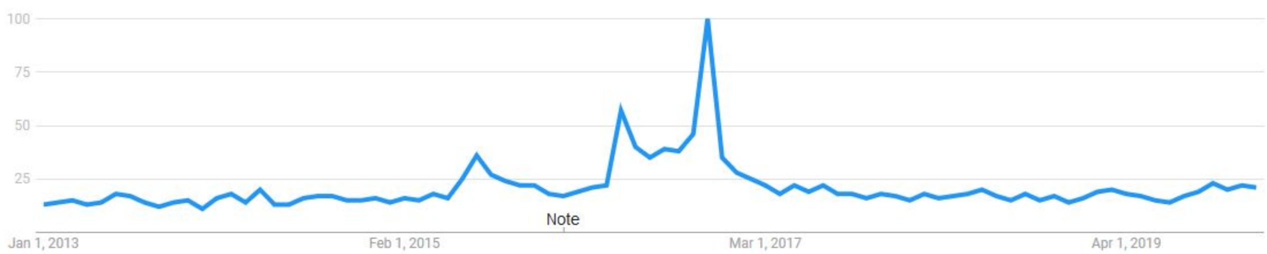
(b) Placebo Test of the Effect Within Hourly Distribution

Notes: Figure (a) shows the net flow into each bin of the hourly distribution from all other bins in the hourly distribution, between April and December of each year. Figure (b) estimates the cumulative effect of the OT exemption threshold policy on the net flow of workers within the hourly distribution of each year between 2012 and 2016. The effect at each bin is estimated via equation **??** and the lines are the running sum of the effects across bins.

Appendix B. History of the 2016 FLSA Policy

The first public announcement of the Department of Labor's intent to update the FLSA overtime exemption threshold occurred on March 13, 2014. After identifying problems with the existing threshold, President Obama declared "I'm directing Tom Perez, my Secretary of Labor, to restore the common-sense principle behind overtime... we're going to consult with both workers and businesses as we update our overtime rules" (White House Archives - March 13, 2014). The reaction from the press was that "Mr. Obama's decision to use his executive authority to change the nation's overtime rules is likely to be seen as a challenge to Republicans in Congress, who have already blocked most of the president's economic agenda" (NYT March 14, 2014). However, while there was an expectation of resistance from Congress, Google search trends suggest that the FLSA overtime exemption policy did not receive much attention from the public at this time (see figure B.1).

Appendix Figure B.1: Google Search Popularity for the Term "FLSA Overtime"



Notes: This figure shows the relative popularity of "FLSA Overtime" as a Google search term between January 2013 and January 2020. A value of 100 indicates its highest popularity level, and the measure of popularity is scaled proportional to this instance.

Interest in the the FLSA grew in 2015 following the DOL's announcement on June 26th that it would like to "raise the threshold under which most salaried workers are guaranteed overtime to equal the 40th percentile of weekly earnings for full-time salaried workers. As proposed, this would raise the salary threshold from \$455 a week (\$23,660 a year) – below the poverty threshold for a family of four – to a projected level of \$970 a week (\$50,440 a year) in 2016" (White House Archives June 30, 2015). Consistent with the normal rulemaking process, the Department of Labor stated that it would release a finalized rule the next year after reviewing comments from the public regarding its current proposal. Similar to the initial announcement in 2014, new articles at the time believed that the policy would face challenges in the courts (NYT June 30, 2015). There were also some reports that companies were already investing in new software to comply with the policy (WSJ Jul 21, 2015), though I do not observe any evidence of this adjustment in the data (compare the difference-distributions in figure 2b).

The finalized threshold of $913 per week was announced on May 18, 2016, and was set to go into effect on December 1, 2016 with automatic updating every three years to adjust for inflation.[46] This announcement received considerable attention from employers, as evident from the spike in Google searches for "FLSA Overtime". In response to the new regulation, "Republican lawmakers, who are close to many of the industries that oppose the new rule, have vowed to block it during a mandated congressional review period". However, given Donald Trump's presidential campaign, there was an understanding that repealing the regulation would be a risky political move for the Republican party as it "could exacerbate an already palpable split between Mr. Trump's blue-collar supporters and the party's establishment donors and politicians" (NYT May 18, 2016). Hence, it was not clear at this point that the rule would be repealed.

On September 20, 2016, twenty-one States sued the Department of Labor in federal court in Sherman, Texas. They argued that the new regulation should be nullified for two reasons. First, they claimed that "the FLSA's overtime requirements violate the Constitution by regulating the States and coercing them to adopt wage policy choices that adversely affect the States' priorities, budgets, and services". Second, the states argued that the magnitude of the proposed overtime exemption threshold conflicted with Congress' original intent in the FLSA to exempt "any employee employed in a bona fide executive, administrative, or professional capacity" (State of Nevada et al v. United States Department of Labor et al, Filing 60). While the DOL has historically used both a duties test and a salary test to define these occupations, the States argued that the language of the text indicates that Congress intended for a duties test to be the primary determinant of overtime exemption status, and a salary threshold of $913 effectively supplants the duties test. Under the Chevron deference principle, the new rule would therefore exceed the power given to the Department of Labor by Congress.

Given the lack of media coverage over the court proceedings, it came as a surprise to employers when Judge Amos L. Mazzant III placed a preliminary injunction on the new overtime exemption threshold on November 22, 2016, after agreeing with the plaintiffs' second argument. From a review of newspaper articles at the time, I find no reports on the court

[46] The final rule also raised the threshold for "highly compensated employees" from $100,000 per year to $134,004. Workers above this threshold are subject to a less stringent duties test to be exempt from overtime. I do not find any bunching in response to this component of the policy.

case in the Wall Street Journal or New York Times between the date of the initial court filing and the date of the injunction. While I do find mentions of the lawsuit as part of broader news on the FLSA overtime exemption threshold, none go into any more detail than stating that a case is under way (eg. USA Today Oct. 12, 2016). Consistent with the lack of awareness of the appeal against the new overtime exemption threshold, I see no increase in Google traffic for the term "FLSA Overtime" in September when the initial case was filed, but a large spike in November after its injunction.

Even among individuals aware of the lawsuit, there was the belief that employers should be ready for the December 1st deadline. For example, a story by the Washington Post quoted a senior executive at the National Federation of Independent Business that "employers can't count on a reprieve, and playing chicken with the Dec. 1 deadline 'could be a very expensive mistake"' (Washington Post Oct 20, 2016). Similarly, an attorney interviewed by the Society of Human Resource Management stated that "although it's possible,... employers shouldn't expect a miracle before the Dec. 1 implementation deadline." (SHRM Oct 21, 2016). Overall, there is no indication that employers expected the injunction.

Since employers did not foresee the injunction, many had already implemented changes in anticipation of the policy or followed through with their promises to their workers. For instance, Wal-Mart and Kroger both raised their managers' salaries above the new overtime exemption threshold and did not retract them after the injunction (WSJ Dec 20, 2016). On the other hand, Burger King announced that it would defer its initial plan to convert its salaried manager to hourly in light of the injunction Slate Jan 16, 2017]. Aside from retail and fast food restaurants, anecdotally, the policy also had a large effect on institutions of higher education. The National Institutes of Health (NIH) and many large universities also gave their post-docs raises above the proposed overtime exemption threshold (Science Jan 4, 2017). On the other hand, some institutions such as the University of Maryland and Arizona State University retracted their promises to either pay their employees overtime or increase their salaries (Huffpost June 7, 2017).

Following the preliminary injunction, there was a general belief from judge Mazzant's language that the $913 exemption threshold would not survive. However, it was uncertain how long the judicial process would take and whether the new Trump administration would propose a smaller increase to the overtime exemption threshold (NYT Nov 22, 2016). It became clearer that the new administration had no desire to defend the overtime policy in courts after the nomination of fast-food executive, and critic of overtime regulation, Andrew

Puzder as Labor Secretary on December 8, 2016 (Forbes March 18, 2016). In the end, Andrew Puzder did not receive enough support from the Senate for his confirmation on February 15, 2016 and the position ultimately went to Alexander Acosta. Nevertheless, Acosta reaffirmed employers' priors that the overtime threshold proposed by Obama would never go into effect. When asked about the overtime exemption threshold during his confirmation hearing on March 22, 2017, Acosta stated that "if you were to apply a straight inflation adjustment, I believe the figure if it were updated would be somewhere around \$33,000". The Department of Labor officially dropped its defense of the \$913 threshold in June 2017.

After the DOL abandoned its defense of the \$913 threshold in June 2017, they submitted a new Request for Information on July 27 (DOL June 27, 2017), allowing the public an opportunity to submit their opinions of the overtime exemption threshold. In December 2017, the DOL announced that it plans to propose a new threshold by October 2018, and most employers believed that it would be within the \$30,000-35,000 per year range SHRM March 2018. The DOL officially proposed a new threshold of \$679 per week (\$35,308 per year) on March 7, 2019. After a period of public comments, on September 24, 2019, the DOL finalized the new threshold at \$684 per week. This new threshold went into effect on January 1, 2020 without as much coverage as the 2016 policy (see figure B.1).

Appendix C. Derivation of the Conceptual Framework

C.1 Solving the baseline model

The worker's utility from employment and searching are characterized by the following Bellman equations:

$$(r+\delta)V_e(w,h) = w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + \delta V_n$$

$$rV_n = b + \lambda \int_{V_e(\theta,F)\geq V_n} [V_e(w(\theta,F),h(\theta,F)) - V_n]dG(\theta,F)$$

The firm's present value of profits is given by

$$J = \frac{\theta h^\beta - w + F\cdot \operatorname{sgn}(S)}{r+\delta}$$

I want to express w_0, h_0, S_0, θ_0^*, and V_n in terms of (θ, a, F) and model primitives. First, rearrange the worker's Bellman equation for the value of employment as

$$V_e(w,h) - V_n = \frac{w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n}{r+\delta} \tag{6}$$

Substitute (6) into the Nash bargaining problem and take first order conditions:

$$(w_0,h_0,S_0) = \arg\max_{(w,h,S)} [V_e(w,h)-V_n]^\alpha \Big[\frac{\theta h^\beta - w + F\cdot\operatorname{sgn}(S)}{r+\delta}\Big]^{1-\alpha}$$

$$\text{FOC}_w = \alpha[h^\beta\theta - w - F_S] - (1-\alpha)[w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n] \tag{7}$$

$$\text{FOC}_h = -\alpha a^{-\frac{1}{\epsilon}}h^{\frac{1}{\epsilon}}(h^\beta\theta - w - F_S) + (1-\alpha)\beta h^{\beta-1}\theta(w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n) \tag{8}$$

Rearrange (7) for w_0, then substitute w_0 into (8) to solve for h_0. S_0 is simply the corner solution that maximizes the Nash product.

$$w_0 = \alpha\Big(\theta h_0^\beta + F\cdot\operatorname{sgn}(S_0)\Big) + (1-\alpha)\Big(a^{-\frac{1}{\epsilon}}\frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + rV_n\Big)$$

$$h_0 = \Big(a^{\frac{1}{\epsilon}}\beta\theta\Big)^{\frac{1}{1+\frac{1}{\epsilon}-\beta}}$$

$$S_0 = \arg\max_{S\in\{-1,1\}} F\cdot\operatorname{sgn}(S)$$

Note that at h_0, $\frac{\partial J}{\partial h} = \frac{\partial V_e(w,h)}{\partial h}$. In other words, h_0 maximizes the total surplus of the employment relationship. To solve for $\theta_0^*(a, F)$, substitute (w_0, h_0) into (6) and solve for the value of θ such that the expression equals 0.

$$\begin{aligned}
V_e(w_0, h_0) - V_n &= 0 && \Leftrightarrow \\
w_0 - a^{-\frac{1}{\epsilon}} \frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n &= 0 && \Leftrightarrow \\
\alpha\Big(\theta h_0^\beta + F \cdot \mathrm{sgn}(S_0) - a^{-\frac{1}{\epsilon}} \frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n\Big) &= 0 && \Leftrightarrow \\
\theta_0^* = \left[\frac{rV_n - \alpha F_S}{(a^{\frac{1}{\epsilon}}\beta)^{\frac{\beta}{1+\frac{1}{\epsilon}-\beta}}(1 - \frac{\beta}{1+\frac{1}{\epsilon}})}\right]^{\frac{1+\frac{1}{\epsilon}-\beta}{1+\frac{1}{\epsilon}}}
\end{aligned}$$

Note from the third line that at θ_0^*, the total surplus of the employment relationship equals 0. I will use this property when computing the comparative statics. Lastly, from the Bellman equation representing the worker's value of searching, I numerically solve for V_n

$$\begin{aligned}
rV_n &= b + \lambda \int_{V_e(\theta,F)\geq V_n} [V_e(w(\theta,F), h(\theta,F)) - V_n] dG(\theta, F) \\
&= b + \lambda \int_{\theta \geq \theta_0^*(F,V_n),F} \left[\frac{w_0(\theta, F, V_n) - a^{-\frac{1}{\epsilon}} \frac{h_0(\theta,V_n)^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n}{r+\delta}\right] dG(\theta, F)
\end{aligned}$$

C.2 Solving the model with overtime

Case 1: No Monitoring Costs or Wage Rigidities

Given job characteristics (w, h, S), the firm's profit is given by

$$J = \frac{\theta h^\beta - (1 + \eta_{(w,h,S)})w + F \cdot \mathrm{sgn}(S)}{r+\delta}$$

where

$$\eta_{(w,h,S)} = \begin{cases} \frac{0.5(h-40)}{40} & \text{if } h > 40 \text{ and } S = -1 \\ \frac{1.5(h-40)}{40} & \text{if } h > 40,\ S = 1, \text{ and } w < \bar{w} \\ 0 & \text{otherwise} \end{cases} \tag{9}$$

The worker's value of employment follows

$$(r+\delta)V_e(w,h) = (1+\eta_{(w,h,S)})w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + \delta V_n$$

Following the same steps as in the model without overtime, I solve the Nash bargaining problem

$$(w_1, h_1, S_1) = \arg\max_{(w,h,S)} [V_e(w,h) - V_n]^{\alpha}\Big[\frac{\theta h^{\beta} - (1+\eta_{(w,h,S)})w + F\cdot \mathrm{sgn}(S)}{r+\delta}\Big]^{1-\alpha}$$

$$\mathrm{FOC}_w = \alpha[h^{\beta}\theta - (1+\eta)w - F_S] - (1-\alpha)[(1+\eta)w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n] \quad (10)$$

$$\mathrm{FOC}_h = -\alpha a^{-\frac{1}{\epsilon}}h^{\frac{1}{\epsilon}}\big(h^{\beta}\theta - (1+\eta)w - F_S\big) + (1-\alpha)\beta h^{\beta-1}\theta\big((1+\eta)w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n\big) \quad (11)$$

The Nash bargaining solution is

$$w_1 = \frac{1}{1+\eta_{(w,h,S)}}\Big[\alpha\Big(\theta h_1^{\beta} + F\cdot\mathrm{sgn}(S_1)\Big) + (1-\alpha)\Big(a^{-\frac{1}{\epsilon}}\frac{h_1^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + rV_n\Big)\Big]$$
$$h_1 = \Big(a^{\frac{1}{\epsilon}}\beta\theta\Big)^{\frac{1}{1+\frac{1}{\epsilon}-\beta}}$$
$$S_1 = \arg\max_{S\in\{-1,1\}} F\cdot\mathrm{sgn}(S)$$

Notice that $h_1 = h_0$, $S_1 = S_0$, and $(1+\eta_{(w,h,S)})w_1 = w_0$. In other words, gross pay, weekly hours, and pay classification all remain the same. By extension, θ_* and V_n are the same as in the baseline model.

Case 2: Monitoring Costs

Given job characteristics (w,h,S), the firm's profit is given by

$$J = \frac{\theta h^{\beta} - (1+\eta_{(w,h,S)})w + F\cdot\mathrm{sgn}(S) - C\cdot 1[S=1, w<\bar{w}]}{r+\delta}$$

where C is a constant and $1[S=1, w<\bar{w}]$ is an indicator that equals 1 if $S=1$ and $w<\bar{w}$. The worker's value of employment and search follow the same formulation as case 1. To solve the Nash bargaining problem, I need to compare the Nash product of the interior solution and the corner solution where $w_2 = \bar{w}$.

Interior solution:

$$
\begin{aligned}
w_2 &= \frac{1}{1+\eta_{(w,h,S)}}\left[\alpha\Big(\theta h_1^{\beta} + F_S - C_{1[S=1,w<\bar{w}]}\Big) + (1-\alpha)\Big(a^{-\frac{1}{\epsilon}}\frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + rV_n^{OT}\Big)\right]\\
h_2 &= \Big(a^{\frac{1}{\epsilon}}\beta\theta\Big)^{\frac{1}{1+\frac{1}{\epsilon}-\beta}}\\
S_2 &= \arg\max_{S\in\{-1,1\}} F_S - C_{1[S=1,w<\bar{w}]}
\end{aligned}
$$

where V_n^{OT} is the value of unemployment given the overtime policy.

Corner solution:

$$
\begin{aligned}
w_2 &= \bar{w}\\
h_2 &\text{ solves } \mathrm{FOC}_h|_{w=\bar{w}} = 0\\
\mathrm{FOC}_h|_{h=h_0,w=\bar{w}} &= a^{-\frac{1}{\epsilon}}h_0^{\frac{1}{\epsilon}}[\bar{w}-w_0] > 0\\
S_2 &= 1
\end{aligned}
$$

All salaried jobs with $w_0 \geq \bar{w}$ and all hourly jobs are unaffected by the monitoring cost C. As such, the hours of these jobs are equivalent to that in case 1, and earnings is likewise similar except for an adjustment of $(1-\alpha)(rV_n^{OT} - rV_n)$. For salaried workers with base incomes less than the overtime exemption threshold, one of three outcomes can occur:

1. Interior solution, where $F > 0$ and $2F - C < 0$. This implies that without overtime, the job is salaried $S_0 = 1$, but with overtime, it becomes reclassified as hourly $S_2 = -1$. The hours remain constant $h_2 = h_0$, but the base income becomes $w_2 = \frac{w_0 - 2\alpha F + (1-\alpha)r(V_n^{OT} - V_n)}{1+\eta_{(w,h,S)}}$, and gross income $g = (1+\eta)w_2 = w_0 - 2\alpha F + (1-\alpha)r(V_n^{OT} - V_n)$.
2. Interior solution, where $2F - C \geq 0$. The job's salaried status and hours are the same as in a world without overtime $(h_2, S_2) = (h_0, S_0)$. However, base income decreases to $w_2 = \frac{w_0 - \alpha C + (1-\alpha)r(V_n^{OT} - V_n)}{1+\eta_{(w,h,S)}}$, and gross income to $g = w_0 - \alpha C + (1-\alpha)r(V_n^{OT} - V_n)$.
3. Corner solution. This creates bunching in the base income distribution of salaried workers. Weekly hours cannot be expressed as a closed form solution, but by evaluating the first order condition at $w = \bar{w}$ and $h = h_0$, I show that weekly hours should increase relative to the baseline scenario, and to a first order approximation, the increase in hours is proportional to the increase in income.

The sign and magnitude of $V_n^{OT} - V_N$ for a given worker type a depends on the distribution of (θ, F) and the proportion of workers affected by each of the above three responses. From the worker's Bellman equation, one can express $V_n^{OT} - V_N$ as:

$$[\frac{r+\delta}{\lambda} + \sigma(\Phi_{unaff}) + \alpha(\sigma(\Phi_{rec}) + \sigma(\Phi_{cov})) + \sigma(\Phi_{bun})]r(V_n^{OT} - V_N)]$$
$$= -\int_{\Phi_{rec}} 2\alpha F dG - \int_{\Phi_{cov}} \alpha C dG + \int_{\Phi_{bun}} (\bar{w} - w_0) - \frac{a^{-\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}}(h_2^{1+\frac{1}{\epsilon}} - h_0^{1+\frac{1}{\epsilon}}) dG + \int_{\Phi_{emp}} \Delta V dG$$

where σ is a measure, $\Phi_{unaff} = \{(\theta, F) | w_0 \geq \bar{w} \text{ or } S_0 = -1\}$ is the set of jobs not directly affected by the overtime exemption threshold. Similarly, Φ_{rec} is the set of jobs that are reclassified, Φ_{cov} is the set of jobs that gain coverage, Φ_{bun} is the set of jobs that get bunched, and Φ_{emp} is the set of matches that become jobs in only one of the two scenarios. ΔV is the difference between the value of employment and unemployment. Abstracting away from the last term, if all workers are reclassified or gain coverage, then $V_n^{OT} - V_N < 0$ since workers do not value their pay classification but the added cost to the employer reduces workers' earnings. On the other hand, if all workers are bunched, then $V_n^{OT} - V_N$ is positive if and only if workers value the increase in earnings more than the loss in leisure. To simplify the subsequent discussion, I assume $V_n^{OT} - V_n = 0$, though the predictions hold for a wider range of values.

To determine whether an interior or corner solution solves the Nash bargaining problem for a given (θ, F, a), I compare the Nash products between the two solutions.

$$NP = \frac{1}{r+\delta}[(1+\eta)w_2 - a^{-\frac{1}{\epsilon}} \frac{h_2^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n^{OT}]^{\alpha} \left[\theta h_2^{\beta} - (1+\eta)w_2 + F_S - C_{1[S=1, w<\bar{w}]}\right]^{1-\alpha}$$

Without a closed form for h_2 in the corner solution, one cannot directly compare the two quantities. However, I can predict when the corner solution is more likely to be the optimum given the baseline income w_0. Notice that at the corner solution, the Nash product simplifies to

$$NP_{corner} = \frac{1}{r+\delta}[\bar{w} - a^{-\frac{1}{\epsilon}} \frac{h_2^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n^{OT}]^{\alpha} \left[\theta h_2^{\beta} - \bar{w} + F\right]^{1-\alpha}$$

From the first order condition that determines h_2, I know that the optimal hours with monitoring costs approaches the optimal hours in the baseline case as the baseline income approaches the overtime threshold (i.e. $\lim_{w_0 \to \bar{w}} h_2 = h_0$). Assuming $V_n^{OT} = V_n$, this implies that $NP_{corner} \to NP_0$ as $w_0 \to \bar{w}$, where NP_0 is the Nash product in the baseline case. Since $NP_0 \geq NP$ for every (θ, F, a), it is the case that for each (θ, F, a), there exists a ε such that

$NP_{corner} \geq NP_{interior}$ for every $w_0 \in \{w_0 | \bar{w} - w_0 < \varepsilon\}$.[47] In other words, jobs initially paying close to the threshold are more likely to be bunched.

Given the Nash bargaining solutions, do more or fewer matches get accepted and become employment relative to the benchmark case without overtime? Recall that in the benchmark case, given a (a, F), all matches with quality greater than or equal to $\theta_0^*(a, F)$ are accepted. Furthermore, the total surplus at θ_0^* equaled zero:

$$\theta_0^* h_0^\beta + F \cdot \text{sgn}(S_0) - a^{-\frac{1}{\epsilon}} \frac{h_0^{1+\frac{1}{\epsilon}}}{1 + \frac{1}{\epsilon}} - rV_n = 0$$

where $h_0 = \left(a^{\frac{1}{\epsilon}} \beta \theta_0^*\right)^{\frac{1}{1+\frac{1}{\epsilon}-\beta}}$ and S_0 maximizes the surplus. Given parameters (θ_0^*, a, F) such that $S_0 = 1$ and $w < \bar{w}$, consider how the total surplus with overtime and monitoring costs (TS_{OT}) compare to the total surplus in the benchmark case $(TS_0 = 0)$:

1. If the job is reclassified, then $TS_{OT} = TS_0 - 2F - r(V_n^{OT} - V_n)$
2. If the job remains salaried but not bunched, then $TS_{OT} = TS_0 - C - r(V_n^{OT} - V_n)$
3. If the job is bunched, then $TS_{OT} = TS_0 + \theta_0^*(h_2^\beta - h_0^\beta) - a^{-\frac{1}{\epsilon}}(\frac{h_2^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - \frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}}) - r(V_n^{OT} - V_n)$

If $r(V_n^{OT} - V_n) \geq 0$, then it must be that $TS > TS_0$.[48] A negative match surplus implies that if the job is agreed upon, then either the firm will receive negative profits or the value of searching exceeds the value of the job. Thus, jobs with negative surplus are not accepted. Since the total surplus is monotonically increasing with respect to θ, for each (a, F), there exists a $\hat{\theta} > \theta_0^*$ such that matches are accepted if and only if $\theta \geq \hat{\theta}$.

Case 3: Downward Nominal Wage Rigidity

For the case with downward nominal wage rigidity, it is instructive to study the effects of imposing overtime on hourly and salaried workers in sequence. First, suppose all hourly workers are covered for overtime, and firms cannot offer a wage, $\frac{w}{h}$, lower than in the benchmark case. The Nash bargaining problem is

$$(w_3, h_3, S_3) = \arg \max_{(w,h,S)} [V_e(w, h) - V_n]^\alpha \left[\frac{\theta h^\beta - (1 + \eta_{(w,h,S)})w + F \cdot \text{sgn}(S)}{r + \delta}\right]^{1-\alpha}$$

[47] If $V_n^{OT} \neq V_n$, then $NP_{corner} \to NP_{max}$ as $w_0 \to \bar{w} - (1 - \alpha)r(V_n^{OT} - V_n)$, where $NP_{max} \geq NP$ for every every (θ, F, a). NP_{max} is the solution to the benchmark Nash bargaining problem with no monitoring cost, assuming a search value of V_n^{OT}.

[48] The inequality is true for the third line since h_0 maximizes $\theta h^\beta - a^{-\frac{1}{\epsilon}} \frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}}$.

where

$$\eta_{(w,h,S)} = \begin{cases} \frac{0.5(h-40)}{40} & \text{if } h > 40 \text{ and } S = -1 \\ 0 & \text{otherwise} \end{cases} \tag{12}$$

and $\frac{w}{h} \geq \frac{w_0}{h_0}$ if $S = -1$.

Since the overtime policy only affects hourly workers working above 40 hours, it has no effect on the job characteristics of matches (θ, F, a) that do not meet this criteria in the benchmark case, aside from the indirect effect of changes to the value of continued search, V_n. For matches where $S_0 = -1$ and $h_0 > 40$, the bargaining solution can either adjust along the hours margin (interior solution) or the salaried/hourly margin (corner solution).

The corner solution sets $S_3 = 1$, and solves for (w_3, h_3). The solution to the Nash bargaining problem is similar to that of case 1:

$$w_3 = \frac{1}{1+\eta_{(w,h,S)}}\left[\alpha\Big(\theta h_3^{\beta} - F\Big) + (1-\alpha)\Big(a^{-\frac{1}{\epsilon}}\frac{h_3^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + rV_n\Big)\right]$$
$$h_3 = \Big(a^{\frac{1}{\epsilon}}\beta\theta\Big)^{\frac{1}{1+\frac{1}{\epsilon}-\beta}}$$

This solution is optimal for matches where the relative cost to being salaried, F, is small.

To solve for the interior solution of (w_3, h_3, S_3), substitute $w = \frac{w_0}{h_0}h$ into the Nash bargaining problem and take first order conditions with respect to h:

$$FOC_h = \alpha(1.5\frac{w_0}{h_0} - a^{-\frac{1}{\epsilon}}h^{\frac{1}{\epsilon}})[\theta h^{\beta} - (1.5 - \frac{20}{h})\frac{w_0}{h_0}h - F_S]$$
$$+ (1-\alpha)(\theta\beta h^{\beta-1} - 1.5\frac{w_0}{h_0})[(1.5 - \frac{20}{h})\frac{w_0}{h_0}h - a^{-\frac{1}{\epsilon}}\frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n]$$

Although there is no closed form solution for h_3, I can determine whether h_3 is larger or smaller relative to the case without overtime by evaluating the first order condition at h_0, recalling that $a^{-\frac{1}{\epsilon}}h_0^{\frac{1}{\epsilon}} = \theta\beta h_0^{\beta-1}$:

$$FOC_h|_{h=h_0} = \underbrace{(1.5\frac{w_0}{h_0} - \theta\beta h_0^{\beta-1})}_{>0 \text{ if } \beta<1.5\alpha}\ \underbrace{(\frac{20}{h_0} - 0.5)}_{<0 \text{ since } h_0>40}\ \underbrace{w_0}_{>0}$$

Substituting in w_0, I show that $\beta < 1.5\alpha$ is a sufficient condition for the first order condition to be negative. Intuitively, the overtime premium raises the income of the worker, so the firm is able to demand longer hours. However, if the worker receives a sufficiently large portion of the surplus relative to the gain in production, then the firm would rather decrease hours to reduce costs.

Assuming that $\beta < 1.5\alpha$, the model predicts a spike at 40 hours per week among hourly workers. Since this model relies on wages being downward rigid, one might expect there to be a higher propensity to bunch among minimum wage workers. In addition, the model predicts that some jobs will be reclassified from hourly to salaried. Following the same argument in case 2, some matches will also fail to become employment.

Next, I extend overtime coverage to salaried workers with weekly base incomes less than an overtime exemption threshold $\bar{w}$. I introduce "wage" rigidity by restricting salaried workers' weekly base pay to be greater than or equal to the agreed upon amount in the benchmark scenario (i.e. $w_3 \geq w_0$). Following the same argument as above, weekly hours decreases for covered salaried workers if $\beta < 1.5\alpha$. Similar to the argument in case 2, the Nash product for individuals initially earning just below the threshold ($\bar{w} - w_0$ is small) is maximized by bunching their weekly income at the threshold. Without a fixed monitoring cost though, the bunching in hour and weekly income only affects salaried workers who are initially working over 40 hours per week ($h_0 > 40$). If reducing hours or increasing base income results in a negative surplus, the job is dissolved.

In contrast to case 2, there are no longer any incentives to reclassify workers who are salaried in the benchmark case ($S_0 = 1$) since the costs of overtime are the same regardless of their classification. However, given that salaried workers gain coverage after hourly workers, the comparative statics should be made relative to the scenario where only hourly workers are eligible for overtime. In that case, workers who are salaried as a result of the asymmetric overtime costs ($S_0 = -1$ and $S_3 = 1$) will be reclassified back to hourly status with shorter hours, or bunched at the threshold, or unemployed. I expect to observe this reclassification effect only for workers earning below the overtime exemption threshold. The asymmetric overtime costs for those above the threshold still incentivizes firms to reclassify hourly workers as salaried.

Appendix D. Normalizing Compensation Variables

D.1 Base Pay and Gross Pay

While the measure of base pay that the Department of Labor uses to determine overtime eligibility is denominated at the weekly level, workers' standard rate of pay is recorded at the paycheck level and their gross pay is calculated at the monthly level. In this section, I explain the procedure I use to normalize these two key measures of compensation in the data to the weekly level. Table D.1 shows the share of workers with each pay frequency in April 2016, and the formula used to compute their base pay and gross pay.

Appendix Table D.1: Normalizing Compensation to Weekly Level, by Pay Frequency

	Share of Workers			
Pay Frequency	Hourly	Salaried	Standard Pay	Gross Pay
Weekly	0.24	0.06	S	$\frac{1}{N}Y$
Biweekly	0.66	0.53	$\frac{1}{2}S$	$\frac{1}{2N}Y$
Semimonthly	0.09	0.35	$\frac{24}{52}S$	$\frac{12}{52}Y$
Monthly	0.01	0.06	$\frac{12}{52}S$	$\frac{12}{52}Y$
All workers	0.66	0.34		

Notes: The first column shows the four frequencies at which individuals can receive their paycheck. Columns 2 and 3 show the share of hourly and salaried workers with each pay frequency, respectively, in April 2016 who are paid according to each pay frequency. Column 4 shows the formula to normalize salaried workers' standard rate of pay, denoted by S, to weekly base pay for each pay frequency. Column 5 shows the formula to normalize monthly gross pay, denoted by Y, to an average weekly gross pay conditional on receiving N paychecks in the month.

To derive workers' weekly base pay from their standard rate of pay, I follow the rules set by the Department of Labor and scale each worker's standard rate of pay by their pay frequency (i.e. $\frac{\text{standard pay}}{\text{week}} = \frac{\text{standard pay}}{\text{paycheck}} \cdot \frac{\text{paycheck}}{\text{weeks}}$). For workers paid weekly or biweekly, I simply multiply the standard rate of pay by 1 and 0.5, respectively, to compute their weekly base pay. For workers paid semimonthly or monthly, the DOL's formula makes the approximation that each month is 1/12 of the year and each year has 52 weeks. Thus, weekly base pay equals standard rate of pay times $\frac{24}{52}$ for workers paid semimonthly, and standard rate of pay times $\frac{12}{52}$ for workers paid monthly.

To express the gross pay variable at the weekly level, I normalize it by the number of paychecks they receive each month and the number of weeks covered per paycheck.

$$\frac{\text{gross pay}}{\text{week}} = \frac{\text{gross pay}}{\text{month}} \Big/ \left(\frac{\text{paychecks}}{\text{month}} \cdot \frac{\text{weeks}}{\text{paycheck}}\right)$$

The normalization is simple for workers paid monthly or semimonthly. By definition, $\frac{\text{paychecks}}{\text{month}} = 1$ for workers paid monthly and $\frac{\text{paychecks}}{\text{month}} = 2$ for workers paid semimonthly. For all workers, the term $\frac{\text{paycheck}}{\text{weeks}}$ is equivalent to the scaling factor used to translate the standard rate of pay to weekly base pay.

The challenge is to impute the number of paychecks that each weekly and biweekly paid worker receives each month. For a given worker-month, this depends on both the day of the week that the worker gets paid, and the number of times that day appears in the month. For instance, if a worker gets paid on a Thursday every two weeks, then the worker's gross pay may include 3 paychecks in December 2016 when there were 5 Thursdays, but only 2 paychecks in April 2016. To illustrate this problem, I plot in figure D.1a the monthly gross pay for a balanced panel of workers who earn between \$455 and \$913 base pay in April 2016, by their pay frequency. Not only do biweekly and weekly paid workers experience spikes in their gross pay, the peaks and troughs do not occur on the same months between years. In contrast, monthly and semi-monthly paid workers only experience a large spike in December of each year, likely reflecting bonuses.

While different workers may receive an extra paycheck in different months, employees of the same firm tend to receive a paycheck on the same day of the month, conditional on their pay frequency. To impute the number of paychecks per month that each firm issues in a month, I apply the following algorithm:

1. Compute the average gross pay across all workers of the same pay frequency within each firm-month.

2. Within each year, for each firm-frequency, compute the median of the average gross pays across the 12 months.

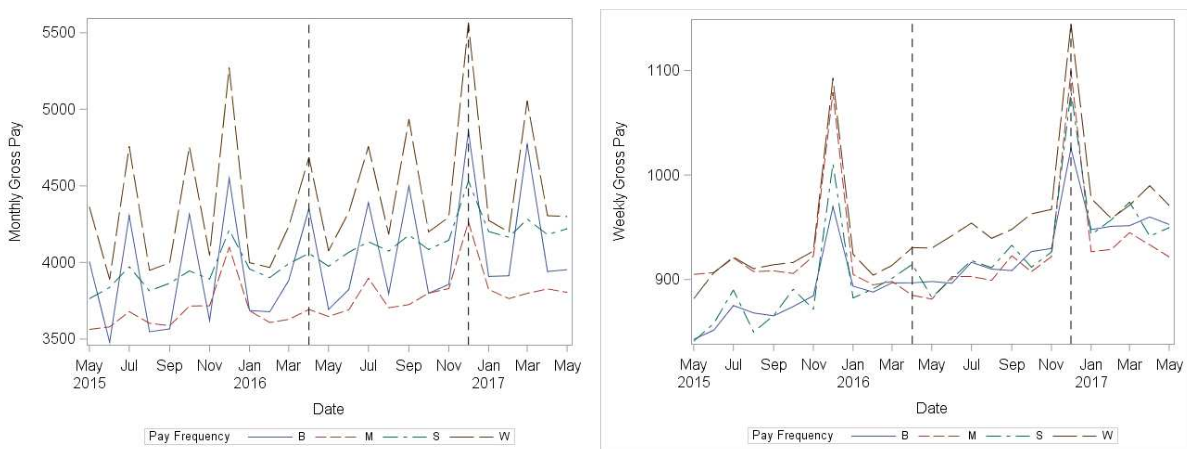
3. I record biweekly workers as receiving 3 paychecks in months where the average gross pay in their firm-frequency exceeds 1.2 times the firm's median gross pay in that year, and 2 otherwise.

4. I record weekly workers as receiving 5 paychecks in months where the average gross pay in their firm-frequency exceeds 1.075 time the firm's median gross pay in that year,

and 4 otherwise.

By computing the number of paychecks at the firm level, I can impute the number of paychecks received by workers who are only employed for a few months. Plotting workers' gross pay, normalized to a weekly level using their imputed number of paychecks, I show in figure D.1b that the periodic spikes in gross pay among biweekly and weekly paid workers disappear. While a spike remains in December, reflecting real increases pay at the end of the year, since this occurs every year, it does not affect my empirical analysis which relies on between year comparisons.

Appendix Figure D.1: Gross Income, by Pay Frequency



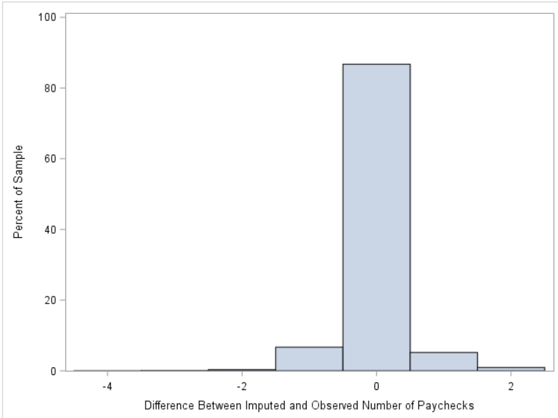
(a) Monthly Gross Pay

(b) Normalized Weekly Gross Pay

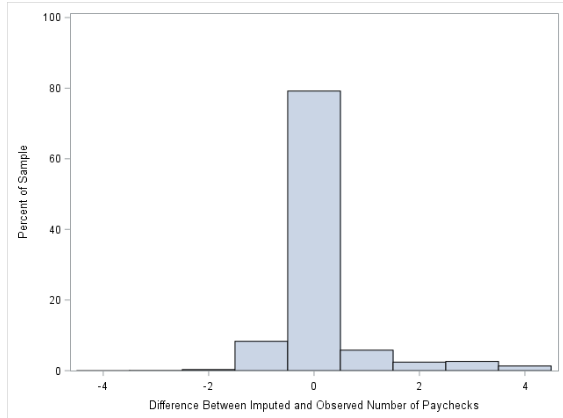
Notes: Panel (a) shows the average monthly gross pay for a balanced panel of workers who earned between \$455 and \$913 per week in April 2016. The pay frequencies from left to right are biweekly, monthly, semi-monthly, and weekly. Panel (b) shows the normalized weekly gross pay for the same panel of workers.

To validate the imputation exercise, I compare the imputed number of paychecks per month to the actual number of paychecks per month, which is recorded from 2016 onwards (see figure D.2). I find that I am able to match the actual number of paychecks for nearly 90% of biweekly paid worker-months and 80% of weekly paid worker-months.

Appendix Figure D.2: Impute Number of Pay Checks, by Pay Frequency



(a) Biweekly



(b) Weekly

Notes: Panel (a) shows distribution of the difference between imputed and actual number of paychecks per month, for all worker-months in 2016 where the worker is paid biweekly. Panel (b) shows a similar distribution for workers who are paid weekly.

D.2 Overtime Pay

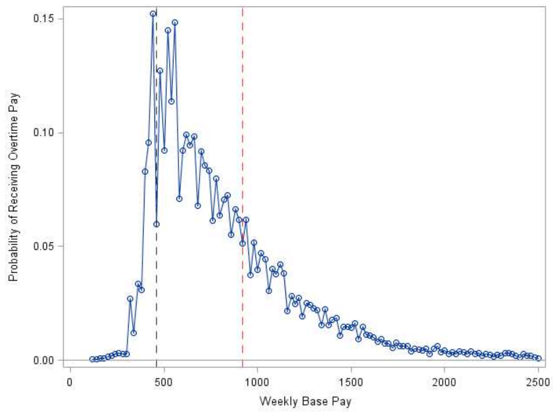
There are two challenges to inferring workers' overtime pay from the ADP data. First, firms are not required to input a value into the "OT earnings" variable. Although the ADP data contains four separate earnings variables and four corresponding hours variables, each capturing a different component of gross compensation, firms are only required to report employees' gross pay and standard rate of pay. Thus, it is uncertain whether a missing "OT earnings" means the firm does not record the value or the worker did not receive any overtime pay. I find that "OT earnings" is non-missing (non-zero) for 45% (X%) of hourly workers and 3.5% (Y%) of salaried workers in April 2016. The second challenge with measuring workers' overtime pay is that the type of compensation included into the "OT earnings" variable is at the discretion of the firm. For example, while one firm may use the variable to record overtime, another firm may use it to record any compensation aside from base pay. In this section, I present the procedure I use to determine each individual's overtime pay from the "OT earnings" variable and its corresponding hours, when available.

I impute overtime pay following the methodology described by Grigsby *et al.* (2019). First, I define an implied wage as the ratio between the "OT earnings" and "OT hours" variables. Next, I divide the implied wage by workers' actual wage to compute an implied overtime premium (i.e. $\frac{\text{OT earnings}}{\text{OT hours}*\text{wage}}$), where a salaried worker's "wage" for overtime purposes is defined by the Department of Labor as $\frac{weeklybase}{40}$. I find that the distribution of the implied overtime premium exhibits significant bunching at 1, 1.5, and 2. In particular, among workers with non-missing "OT earnings", 75% of hourly workers and 79% of salaried workers have implied overtime premiums within 1.4-1.6 and 1.9-2.1. For these workers, the "OT earnings" variable is likely representative of the earnings they received for all hours worked above 40 in each week of the month. Since hourly workers receive their regular rate of pay regardless of the overtime premium, I define their monthly overtime pay as the difference between "OT earnings" and the amount they would earn if there was no overtime premium (i.e. OT pay = OT earnings − hours∗wage). For salaried workers, their monthly overtime pay is equal to "OT earnings". I normalize my measure of monthly overtime pay to the weekly level following the same procedure for monthly gross pay as described in the previous subsection.

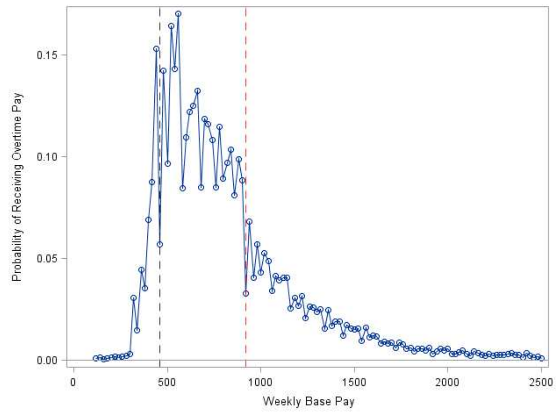
To validate my measure of monthly overtime pay, figure XX plots it against a variable that records total monthly hours. As a comparison, I also include in the figure the implied monthly overtime pay of workers' whose implied overtime premium is outside of 1.4-1.6 and 1.9-2.1.

I find that 45% of hourly workers receive non-zero overtime pay in April 2016, which is 13 p.p. greater than the result by Grigsby *et al.* (2019) who use a random sample of all worker-years between May 2008 and December 2016. Among hourly workers who receive overtime, it accounts for 8.2% of their cumulative gross earnings. In contrast, only 3.5% of salaried workers received overtime pay in April 2016. To validate my measure of overtime for salaried workers, I plot in figure D.3 the probability that a salaried worker receives overtime as a function of their weekly base pay. Consistent with compliance to the overtime regulation, and potentially selection into bunching, salaried workers earning less than the overtime exemption are far more likely to receive overtime pay compared to those earning above it. Furthermore, the probability of receiving overtime in FLSA states in December 2016, and California and New York in April 2016, exhibits a discontinuous drop in at exactly the threshold.

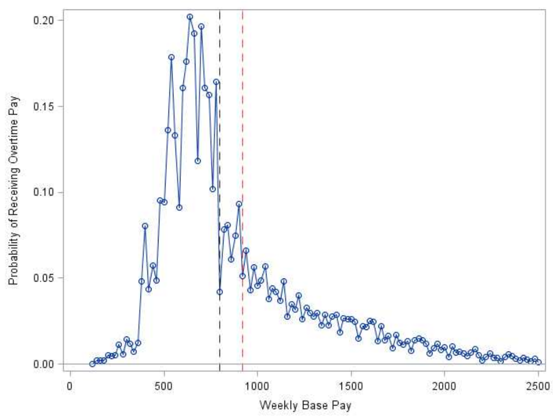
Appendix Figure D.3: Probability of Receiving Overtime Pay, Conditional on Base Pay



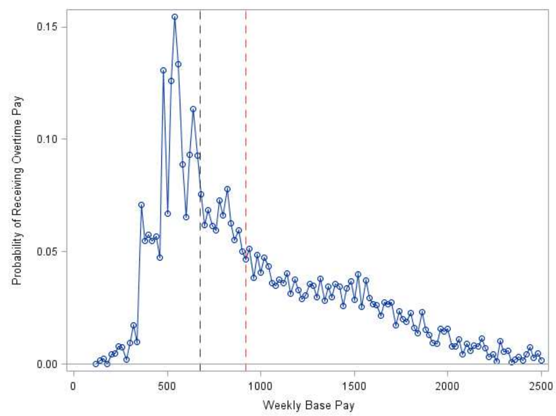
(a) FLSA states, April 2016



(b) FLSA states, Dec 2016



(c) California, April 2016



(d) New York, April 2016

Notes: Each graph shows the probability that salaried workers receive non-zero overtime pay in the month, as a function of their weekly base pay. The sample in figure (a) is restricted to salaried workers not living California, New York or Alaska, in the month April 2016. The sample in figure (b) is restricted to salaried workers in the same states as figure (a) in December 2016. The sample in figure (c) is restricted to salaried workers in California in April 2016. The sample in figure (d) is restricted to salaried workers in New York in April 2016.

Appendix E. Derivation of Estimator in Equation 3

If the coefficients in equation 2 satisfy

$$\beta_{jk} = 0 \text{ for every } k \geq k^*$$

$$\alpha_{jkt} = \gamma_1 \alpha_{jk,t-1} + \gamma_0$$

then for every $k < k^*$, an unbiased estimator of β is

$$\begin{aligned}\hat{\beta}_{jk} &= \left(\bar{N}_{jk,Dec,t} - \bar{N}_{jk,Apr,t}\right) - \hat{\gamma}_1\left(\bar{N}_{jk,Dec,t-1} - \bar{N}_{jk,Apr,t-1}\right) - \hat{\gamma}_0 \\ &= \Delta\bar{N}_{jkt} - \hat{\gamma}_1\Delta\bar{N}_{jk,t-1} - \hat{\gamma}_0\end{aligned}$$

where $\bar{N}_{jkmt}$ is the average N_{ijkmt} across all firms, and $\hat{\gamma}_1$ and $\hat{\gamma}_0$ are estimated using all salaried workers in bins $k \geq k^*$ from

$$\Delta\bar{N}_{sal,kt} = \gamma_1\Delta\bar{N}_{sal,k,t-1} + \gamma_0 + \epsilon_{sal,kt}$$

Proof. For every $k \geq k^*$,

$$\begin{aligned}\bar{N}_{jk,Dec,t} &= \bar{N}_{jk,Apr,t} + \alpha_{jkt} \\ \Rightarrow \qquad \Delta\bar{N}_{jkt} &= \alpha_{jkt} \\ \Rightarrow \qquad \Delta\bar{N}_{jkt} &= \gamma_1\alpha_{jk,t-1} + \gamma_0 \\ \Rightarrow \qquad \Delta\bar{N}_{jkt} &= \gamma_1\Delta\bar{N}_{jk,t-1} + \gamma_0\end{aligned}$$

This implies that I can estimate γ_1 and γ_0 by regressing $\Delta\bar{N}_{sal,kt}$ on $\Delta\bar{N}_{sal,k,t-1}$ using all bins $k \geq k^*$. Given the γ's, I can then predict the α_{jkt}'s for both salaried and hourly workers with bins $k < k^*$.

$$\hat{\alpha}_{jtk} = \hat{\gamma}_1\Delta\bar{N}_{jk,t-1} + \hat{\gamma}_0$$

From equation 2, I estimate the β_{jk}'s as the difference between $\Delta\bar{N}_{jkt}$ and $\hat{\alpha}_{jkt}$. ∎

Appendix F. Properties of the Flow Decomposition

From the perspective of a given firm i, each worker can be in any one of $N = N_1 + N_2 + 1$ states: employed in one of the N_1 bins of base pay in the salaried distribution, employed in one of the N_2 bins of base pay in the hourly distribution, or not employed at that firm. Denote the number of workers in state $q \in \mathbb{R}^N$ at month t by n_t^q. Between any two months, t_0 and t_1, a worker in state $q \in \mathbb{R}^N$ has a probability p_{qr} of transitioning to state $r \in \mathbb{R}^N$. Given these probabilities and the number of workers in each state in month t_0, I can write the number of of workers in each state in month t_1 using a transition matrix:

$$\begin{pmatrix} n_{t_1}^s \\ n_{t_1}^h \\ n_{t_1}^u \end{pmatrix} = \begin{pmatrix} p_{ss} & p_{hs} & p_{us} \\ p_{sh} & p_{hh} & p_{uh} \\ p_{su} & p_{hu} & p_{uu} \end{pmatrix} \begin{pmatrix} n_{t_0}^s \\ n_{t_0}^h \\ n_{t_0}^u \end{pmatrix}$$

where each element of the matrix is itself a transition matrix. For instance, $p_{ss} = (p_{s_j s_k}) \in \mathbb{R}^{S_1 \text{x} S_1}$ is a matrix of transition probabilities across the different bins within the salaried distribution. All the other elements of the transition matrix are similarly defined.

Using the cross-sectional aspect of the data, I observe the number of workers within each bin of the salaried and hourly distribution in month t_0 and t_1. From the panel structure of the data, I also observe the number of workers that transition from state q to r between any two months: $p_{qr} n_{t_0}^q$. To decompose changes in the cross-section into these flows, I expand the right hand side of the transition matrix equation. For concreteness, I consider a specific bin j within the salaried distribution, but the argument is the same for any state $q \in \mathbb{R}^N$.

$$\begin{aligned}
n_{j,t_1}^s &= \sum_k p_{s_k,s_j} n_{k,t_0}^s + \sum_k p_{h_k,s_j} n_{k,t_0}^h + p_{u,s_j} n_{t_0}^u \\
&= p_{s_j,s_j} n_{j,t_0}^s + \sum_{k \neq j} p_{s_k,s_j} n_{k,t_0}^s + \sum_k p_{h_k,s_j} n_{k,t_0}^h + p_{u,s_j} n_{t_0}^u \\
&= (1 - \sum_{k \neq j} p_{s_j,s_k} - \sum_k p_{s_j,h_k} - p_{s_j,u}) n_{j,t_0}^s + \sum_{k \neq j} p_{s_k,s_j} n_{k,t_0}^s + \sum_k p_{h_k,s_j} n_{k,t_0}^h + p_{u,s_j} n_{t_0}^u
\end{aligned}$$

Subtracting both sides by n_{j,t_0}^s, the difference in the number of salaried workers at bin j between t_0 and t_1 is equal to

$$
\begin{aligned}
\Delta n_j^s &= n_{j,t_1}^s - n_{j,t_0}^s \\
&= (-\sum_{k\neq j} p_{s_j,s_k} - \sum_k p_{s_j,h_k} - p_{s_j,u}) n_{j,t_0}^s + \sum_{k\neq j} p_{s_k,s_j} n_{k,t_0}^s + \sum_k p_{h_k,s_j} n_{k,t_0}^h + p_{u,s_j} n_{t_0}^u \\
&= \underbrace{(p_{u,s_j} n_{j,t_0}^u - p_{s_j,u} n_{j,t_0}^s)}_{\text{Net Employment } (\Delta n_j^{emp,sal})} + \underbrace{\sum_k (p_{h_k,s_j} n_{k,t_0}^h - p_{s_j,h_k} n_{j,t_0}^s)}_{\text{Net Reclassification } (\Delta n_j^{reclass,sal})} + \underbrace{\sum_{k\neq j} (p_{s_k,s_j} n_{k,t_0}^s - p_{s_j,s_k} n_{j,t_0}^s)}_{\text{Within-Classification } (\Delta n_j^{within,sal})}
\end{aligned}
$$

Each component of the above decomposition satisfy three intuitive identities. First, the sum of flows within a distribution equals zero (i.e. if bin j gains a worker from bin k, then bin k loses a worker to bin j):

$$
\begin{aligned}
\sum_j \Delta n_j^{within} &= \sum_j \sum_{k\neq j} (p_{s_k,s_j} n_{k,t_0} - p_{s_j,s_k} n_{j,t_0}) \\
&= 0
\end{aligned}
$$

Second, the sum of flows from salaried to hourly, and hourly to salaried equals zero (i.e. if the salaried distribution gains a worker from the hourly, then the hourly distribution loses a worker to the salaried):

$$
\begin{aligned}
\sum_j (\Delta n_j^{reclass,sal} + \Delta n_j^{reclass,hr}) &= \sum_j \Big(\sum_k (p_{h_k,s_j} n_{k,t_0}^h - p_{s_j,h_k} n_{j,t_0}^s) + \sum_k (p_{h_k,s_j} n_{k,t_0}^s - p_{s_j,h_k} n_{j,t_0}^h) \Big) \\
&= \sum_j \sum_k (p_{h_k,s_j} n_{k,t_0}^h - p_{s_j,h_k} n_{j,t_0}^h) + \sum_j \sum_k (p_{h_k,s_j} n_{k,t_0}^s - p_{s_j,h_k} n_{j,t_0}^s) \\
&= 0
\end{aligned}
$$

Third, the sum of the total change in the number of workers in the salaried and hourly distributions is equal to the sum of the change in net employment.

$$
\sum_j (\Delta n_j^{total,sal} + \Delta n_j^{total,hr}) = \sum_j (\Delta n_j^{emp,sal} + \Delta n_j^{emp,hr})
$$

Appendix G. Robustness to Including the 0.1% Largest Firms

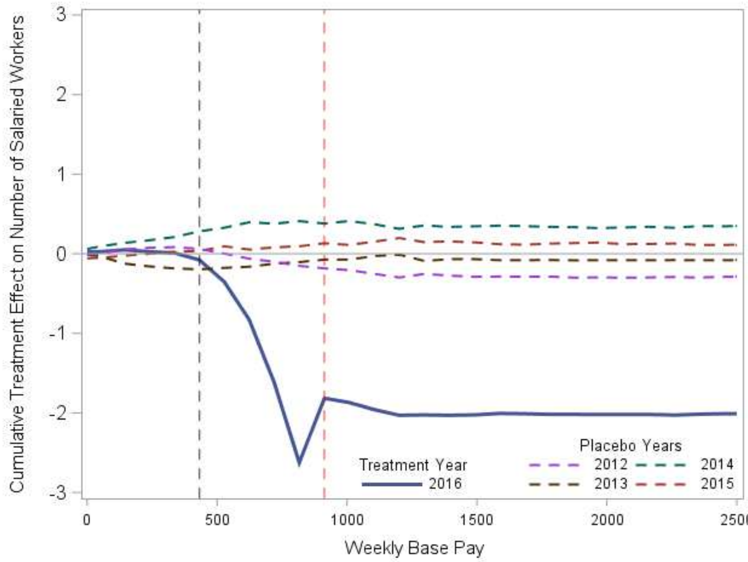
There is a trade-off to dropping the largest firms from the sample. On the one hand, the effect of the policy on medium-sized firms may not be representative of the effect on large firms, and considering that large firms hire a big segment of the labor force, they are an important population to study. On the other hand, since the outcome variables are measured in levels, the estimates of the treatment effects are heavily influenced by any firm-specific policies of large firms.

In this section, I test whether my econometric model is able to control for fluctuations in large firms that are unrelated to the 2016 overtime policy. I use the methodology in section 6.1 to estimate the cumulative treatment effect of the 2016 policy on the frequency distribution of salaried and hourly workers, using the full sample of firms. To validate whether the econometric strategy is appropriate for this larger sample, I conduct a placebo test by estimating the "effect" of the policy on the distributions between 2012 and 2015.

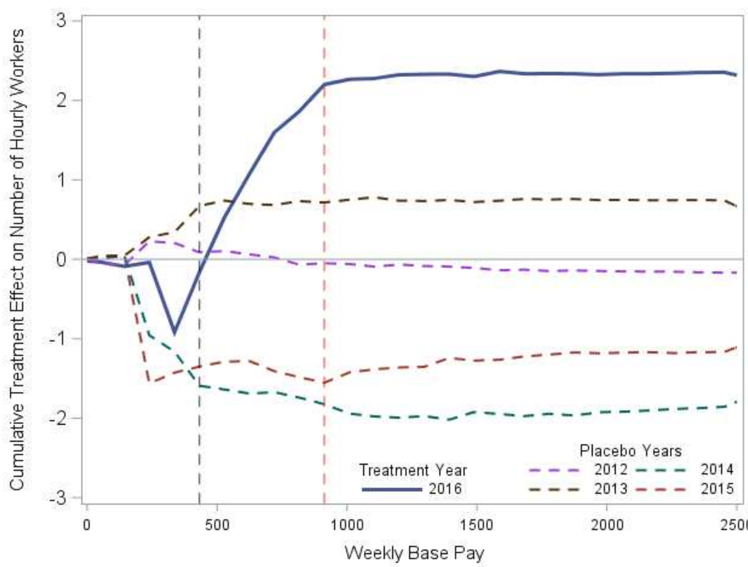
The effect on the salaried distribution appears to be very similar regardless of whether or not I keep the 0.1% largest firms. I present in figure G.1a the cumulative effect of the policy on the number of salaried workers in each year while keeping the large firms. The graph looks remarkably similar to figure 4b where I dropped the largest firms: the policy reduced the number of salary positions in the average firm by about 2 and had no effect in the placebo years prior to 2016.

In contrast, the estimates of the effect on the hourly distribution differ significantly once I include the largest firms. Comparing figure G.1b and 5b, the increase in the number of hourly workers in the full sample is nearly double that of the truncated sample. However, the placebo tests in figure G.1b also show much larger deviations from zero than in figure 5b, particularly below the $455 line. This suggests that the econometric model poorly predicts the counterfactual hourly distribution of large firms. Given the poor fit of the model, it is unclear whether large firms actually responded more strongly to the policy. Thus, my preferred specification drops the 0.1% largest firms.

Appendix Figure G.1: Effect of Raising the OT Exemption Threshold on the Frequency Distribution of Weekly Base Pays by Salaried/Hourly Status, Keeping Firms of All Sizes



(a) Placebo Test on Number of Salaried Workers

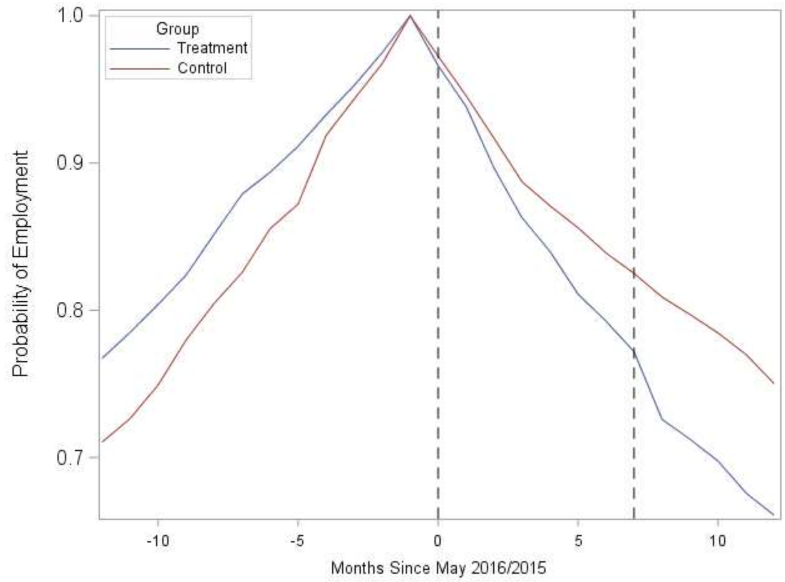


(b) Placebo Test on Number of Hourly Workers

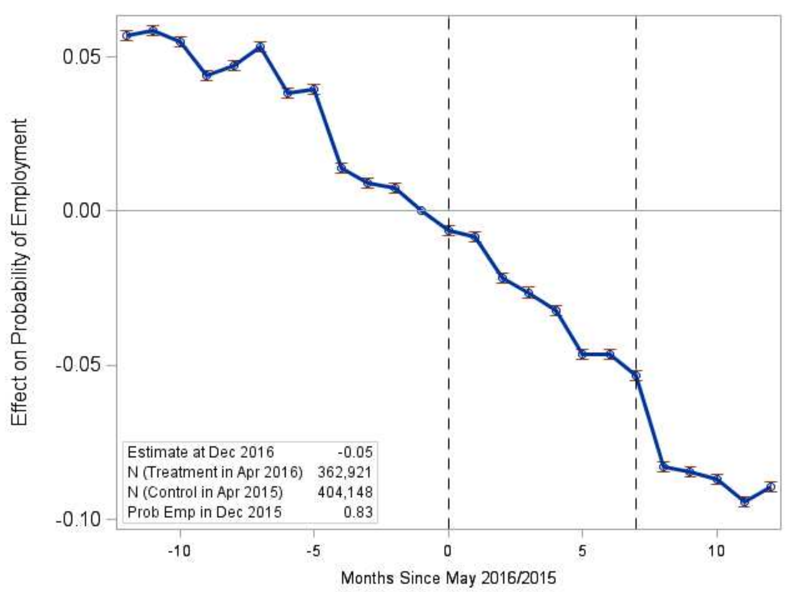
Notes: Panel (a) shows the cumulative effect of raising the OT exemption threshold on the number of salaried workers in December of each year between 2012 and 2016. The treatment effect at each \$96.15 bin is estimated using equation 3. Panel (b) presents the analogous graph for the cumulative effect of the policy on the number of hourly workers. In both graphs, the vertical black and red lines are at the start of the bins that contain the old and new OT exemption thresholds (\$455 and \$913), respectively.

Appendix H. Difference-in-Difference Results

Appendix Figure H.1: Probability of Unemployment Conditional on Workers Earning Between $455 and $913 in April 2016 and April 2015



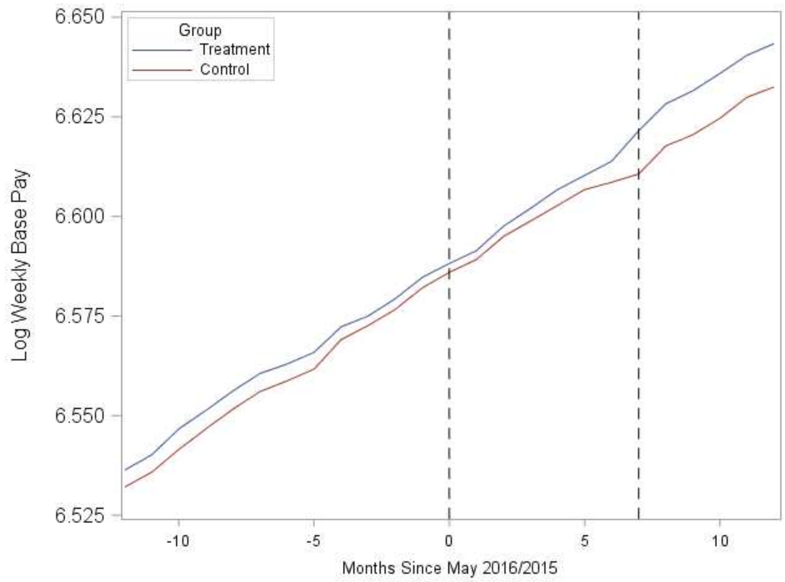
(a) Prob. of Employment, Conditional on Employed at t=0



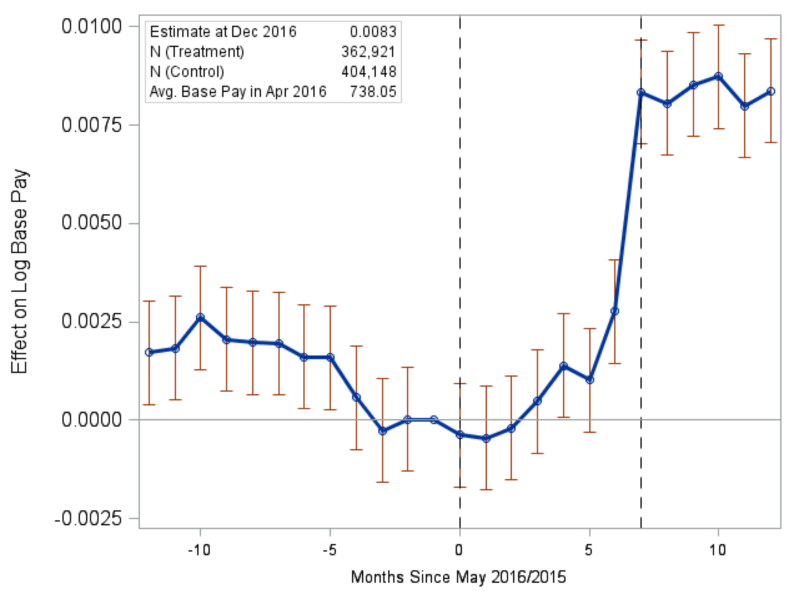
(b) Diff. in Prob. of Employment Between 2016 and 2015

Notes: In panel (a), the dependent variable is a dummy that equals 1 if a worker is employed at the same firm that they were working at in April 2015 (control) or April 2016 (treatment). The treatment group consists of all workers who are paid a salary between $455 and $913 per week in April 2016. The control group are workers who satisfy the same conditions in April 2015. The x-axis shows the number of months since May 2016 for the treatment group, and May 2015 for the control group. The first dashed bar is at 0 months from the event date and the second dashed bar is at 7 months after event date. The second dashed bar corresponds with December 2016 for the treatment group. Panel (b) shows the difference-in-difference estimates between the employment probabilities of the treatment and control groups in panel (a).

Appendix Figure H.2: Evolution of Log Base Pay Conditional on Workers Earning Between $455 and $913 in April 2016 and April 2015



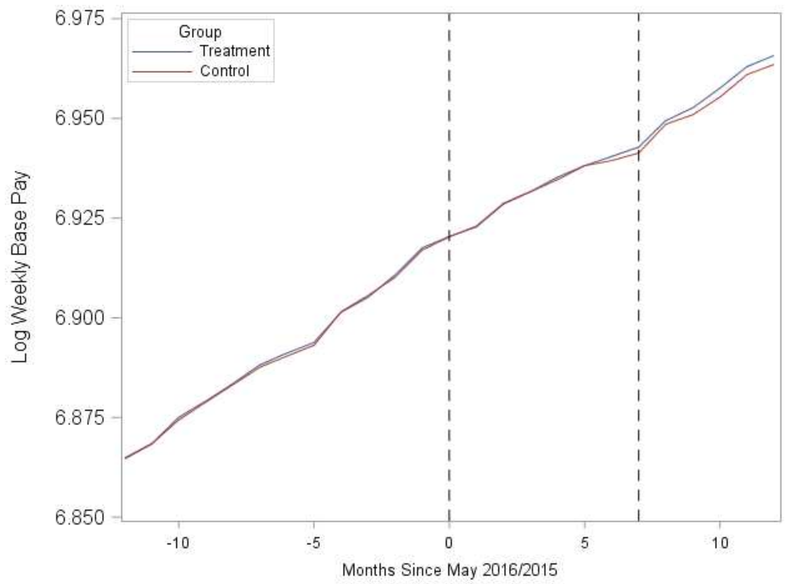
(a) Log Base Pay, Conditional on Employment



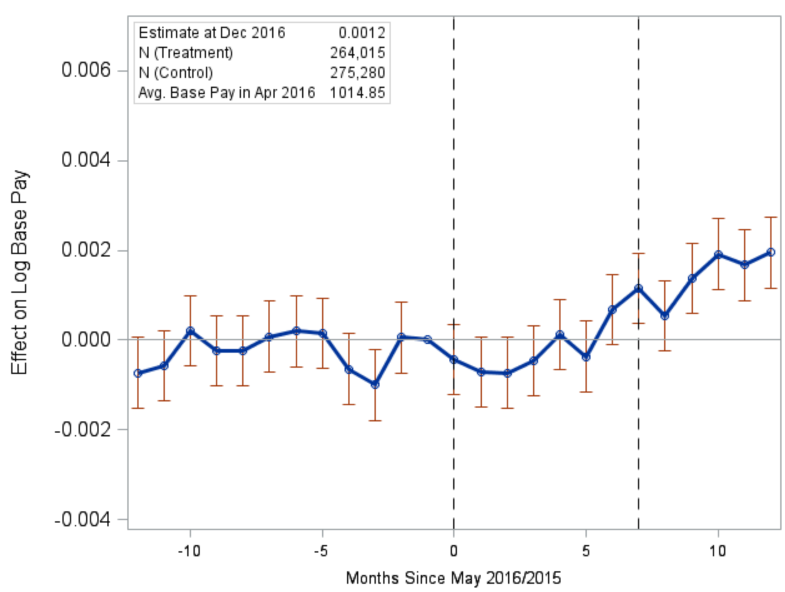
(b) Difference in Log Base Pay Between 2016 and 2015

Notes: In panel (a), the dependent variable equals the natural logarithm of worker's weekly base pay, averaged by workers in the treatment and control groups. The treatment group consists of all workers who are paid a salary between $455 and $913 per week in April 2016, and are continuously employed for a year before and after April 2016. The control group are workers who satisfy the same conditions in 2015. The x-axis shows the number of months since May 2016 for the treatment group, and May 2015 for the control group. The first dashed bar is at 0 months from event time and the second dashed bar is at 7 months after event time. The second dashed bar corresponds with December 2016 for the treatment group. Panel (b) shows the difference-in-difference coefficients on log base pay, estimated using equation 5.

Appendix Figure H.3: Evolution of Log Base Pay Conditional on Workers Earning Between \$913 and \$1113 in April 2016 and April 2015



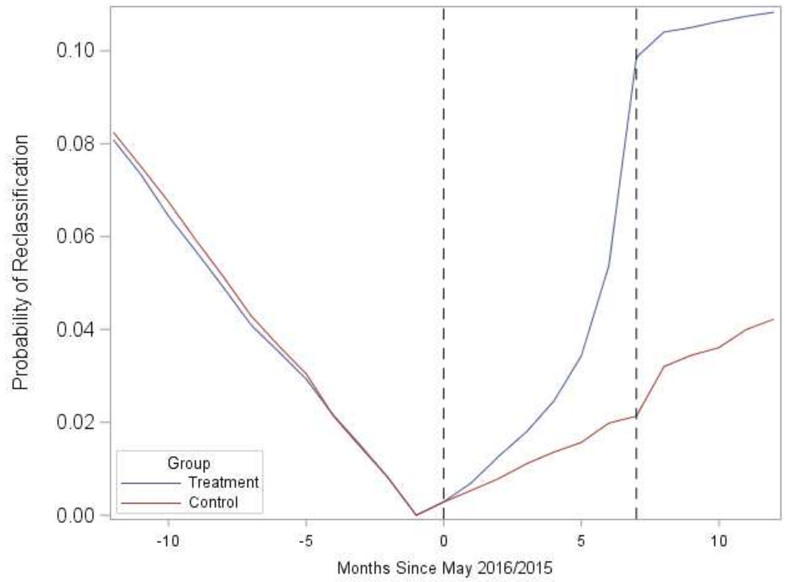
(a) Log Base Pay, Conditional on Employment



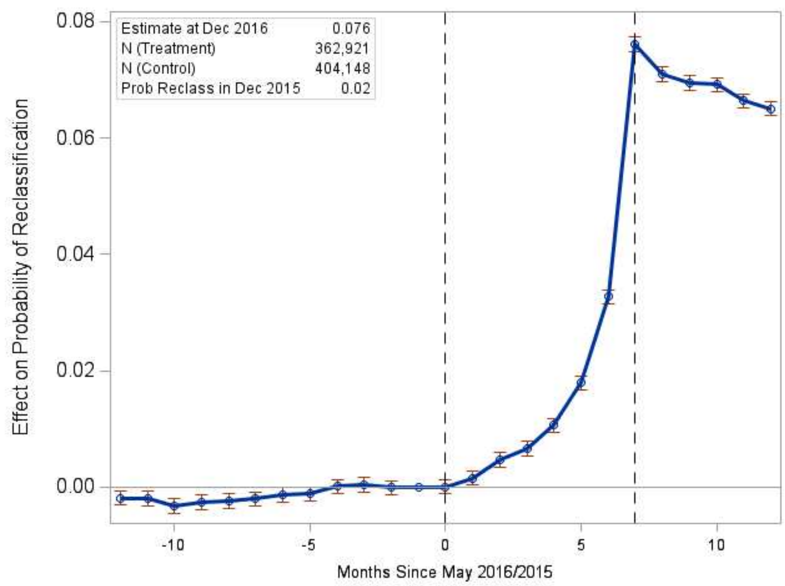
(b) Difference in Log Base Pay Between 2016 and 2015

Notes: In panel (a), the dependent variable equals the natural logarithm of worker's weekly base pay, averaged by workers in the treatment and control groups. The treatment group consists of all workers who are paid a salary between \$913 and \$1113 per week in April 2016, and are continuously employed for a year before and after April 2016. The control group are workers who satisfy the same conditions in 2015. The x-axis shows the number of months since May 2016 for the treatment group, and May 2015 for the control group. The first dashed bar is at 0 months from event time and the second dashed bar is at 7 months after event time. The second dashed bar corresponds with December 2016 for the treatment group. Panel (b) shows the difference-in-difference coefficients on log base pay, estimated using equation 5.

Appendix Figure H.4: Probability of Being Reclassified to Hourly, 2015 vs. 2016



(a) Prob. of Reclassification, Conditional on Employment



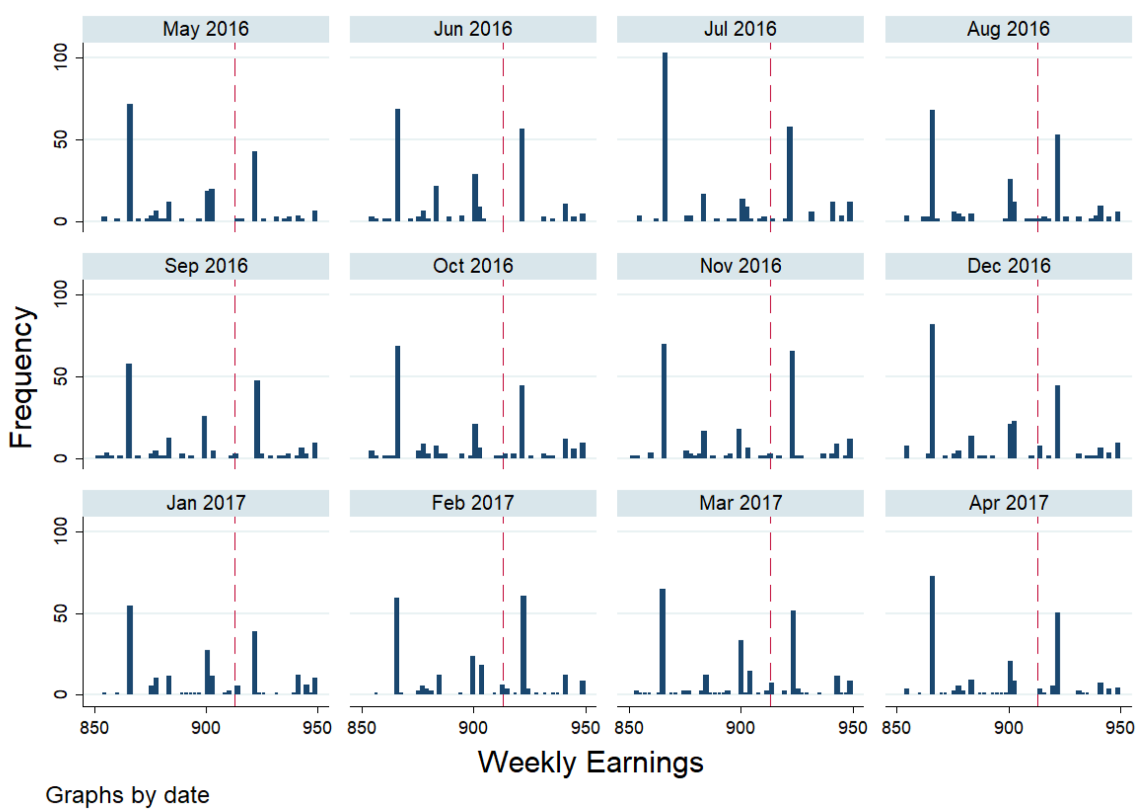
(b) Difference in Probability of Reclassification Between 2016 and 2015

Notes: In panel (a), the dependent variable is a dummy that equals 1 if an individual is an hourly worker. The treatment group consists of all workers who are paid a salary between \$455 and \$913 per week in April 2016, and are continuously employed for a year before and after April 2016. The control group are workers who satisfy the same conditions in April 2015. The x-axis shows the number of months since May 2016 for the treatment group, and May 2015 for the control group. The first dashed bar is at 0 month from event time and the second dashed bar is at 7 months after event time. The second dashed bar corresponds with December 2016 for the treatment group. Panel (b) shows the difference-in-difference coefficients on the probability of reclassification, estimated using equation 5.

Appendix I. Analysis using the Current Population Survey

There are many advantages of the ADP data over traditional survey data. Foremost for the purposes of studying the overtime exemption policy is that it records workers' base salaries without measurement error, for a very large sample of workers. These features make it possible to compare the distribution of salaries over time with minimal concern that differences are driven by measurement error or changes in the sample population. A limitation of the ADP data though is that it does not record the hours worked by salaried workers. Hence, a natural response would be to supplement the main analysis by using survey data, such as the Current Population Survey (CPS), to estimate the effect of raising the overtime exemption threshold on workers' weekly hours. However, I show that the CPS is unable to even pick up the clear bunching and reclassifications effects identified from the ADP data.

Appendix Figure I.1: Frequency Distribution of Salaried Workers in \$2 Bins of Weekly Earnings, by Date



Notes: This figure shows the frequency distribution of respondents' usual weekly earnings in the CPS. The sample is restricted to individuals who are not paid an hourly wage, and earn between \$851 and \$950 per week. The dotted vertical red line is at \$913 per week.

To begin, I plot the frequency distributions of weekly earnings of salaried workers for each month between May 2016 and April 2017 in figure I.1. The number of respondents earning within a dollar of \$913 per week experiences a visibly small jump between November and December 2016 that persists after December.[49] In the year prior to December 2016, 0.09% of salaried workers report earning within a dollar of \$913, whereas in the year after, 0.37% report earning within that interval. However, the "bunching" at the threshold is considerably smaller than the other spikes in the distribution. This is inconsistent with the result in figure 2a wherein the spike at the new overtime exemption threshold is the largest spike along the entire distribution in December 2016.

Replicating figures 2a and 2b, I try to isolate the dip and bunching by taking the difference in the earnings distributions before and after the policy. Given that there are on average only 4,470 salaried workers surveyed per month, I construct the post-policy distribution by pooling all observations between December 2016 and April 2017, and the pre-policy distribution using all observations in the analogous months in the previous year. The two distributions, overlaid in figure I.2 look very similar. Furthermore, the difference between the distributions do not exhibit the clear dip and bunching observed using the ADP data. While there is a drop in the number of salaried workers earning between \$455 and \$912 and an increase in the number of workers earning exactly \$913 from 2015 to 2016, the same is also true from 2014 to 2015. Overall, I am unable to find definitive evidence of large bunching using the CPS data.

The absence of bunching in the CPS data may be attributed to measurement error in the weekly earnings variable. For example, respondents may tend to round their reported earnings to the nearest \$1000 annual income or \$100 weekly income. Alternatively, when asked their "usual" weekly earnings, respondents may report their most common weekly earnings over the past year, rather than their weekly earnings in the month that they are surveyed. Given these concerns over measurement error in reported earnings, the CPS may be more suited to identifying reclassification effects.

In figure I.3, I plot the proportion of respondents earning who report being paid per hour. I find no visible evidence of a trend break in the probability of hourly status between May 2016 and December 2016 for those earning between \$400 and \$1000 per week. To control for date-specific effects, I estimate a difference-in-difference where I assume that the proportion of hourly workers among those earning between \$1000 and \$1200 per week follows the same trend as those earning between \$400 and \$1000 per week. I do not find any effect of the

[49]The "bunching" is actually at \$913.46 per week, corresponding to an annual salary of \$47,500.

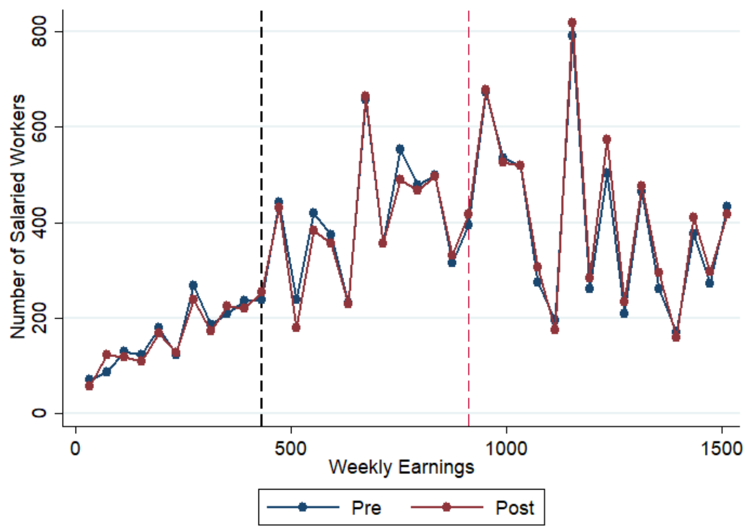
policy on the share of hourly workers under this specification.

One concern with restricting the sample within each cross-section to only workers who earn between \$400 and \$1000 per week is that the policy might affect the selection of workers into this sample. To address this issue, I leverage the panel structure of the CPS data to identify the change within-worker over one year. First, I restrict the sample to workers who, in their first survey, report being non-hourly, and earning between \$455 and \$913. Given that the reclassification and bunching effects estimated from the ADP data are largest in December 2016 (see fig H.4), there should be a jump in the share of hourly workers among those who completed their second survey between December 2016 and February 2016. However, figure I.4 shows no trend break in the share of workers who transition to hourly status in December 2016. Instead, I find a large jump in hourly workers among the September to November 2016 respondents. Comparing salaried workers initially earning between \$455 and \$913 per week to salaried workers initially earning between \$913 and \$1200, I find no statistically significant differences in their probabilities of becoming hourly in December 2016. While not reported, I also find no earnings effect from the cohort-by-cohort difference-in-difference. These observations are inconsistent with the results from the main analysis.

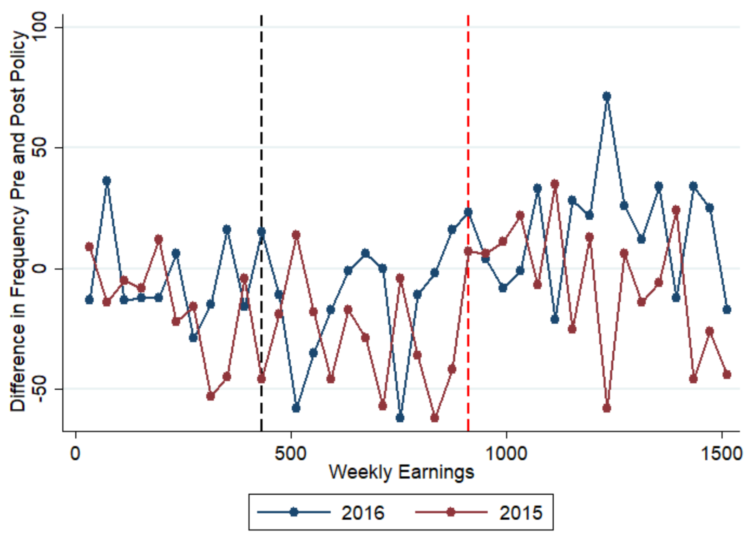
In summary, I am unable to replicate the key results found in the ADP data using the CPS, due to a combination of measurement error and small sample size. Given that the CPS cannot identify the bunching or reclassification effects, it is not surprising that I also do not find any significant changes to weekly hours worked among salaried workers around the time of the policy.[50] Overall, the CPS is simply too imprecise and small to study the effects of raising the overtime exemption threshold on the labor market.

[50]Graphs available upon request.

Appendix Figure I.2: Difference in Distribution of Salaried Workers Before and After Raising the OT Exemption Threshold, Using CPS



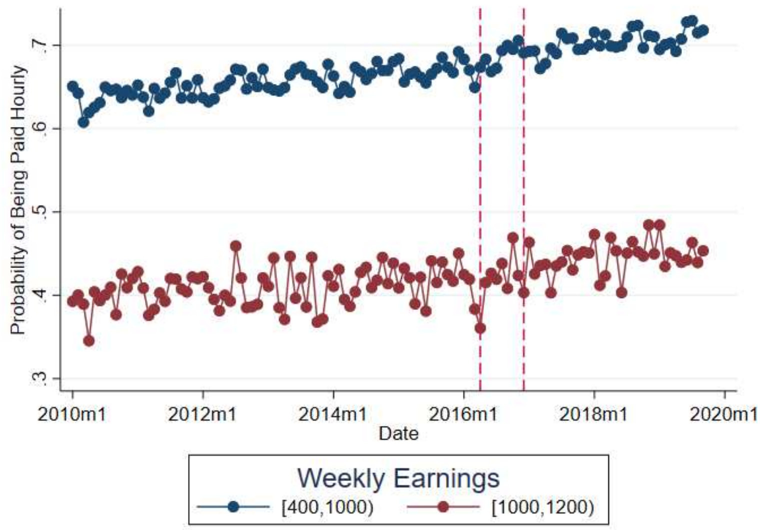
(a) Distribution of Salaried Workers' Weekly Earnings



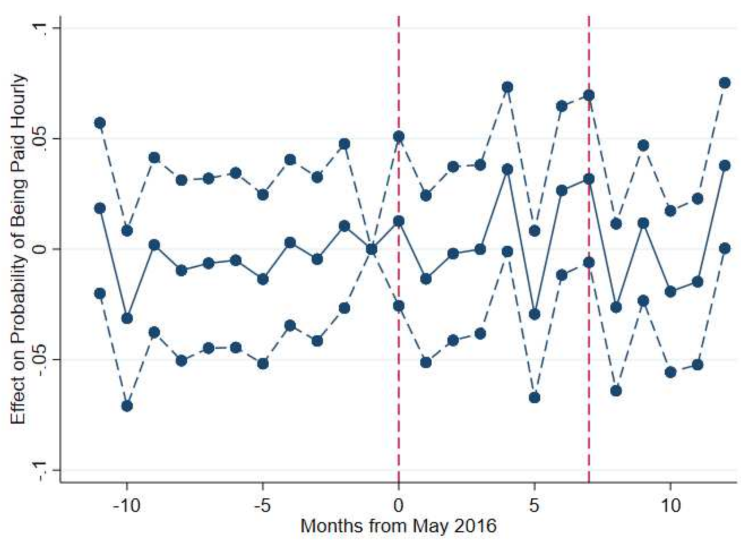
(b) Difference in Distribution Pre and Post Policy

Notes: Panel (a) shows the frequency distribution of salaried workers' weekly earnings in $40 bins, reported in the CPS. The distribution in the pre-period is constructed using all respondents between December 2015 and April 2016. The post-period is constructed using all respondents between December 2016 and April 2015. The "2016" line in panel (b) shows the difference between the pre and post distributions in panel (a). The "2015" line shows the difference between the pre-distribution and the analogous distribution of salaried workers from December 2014 and April 2015.

Appendix Figure I.3: Difference in Difference of Probability of Being Paid Hourly Using Repeated Cross Sections, Using CPS



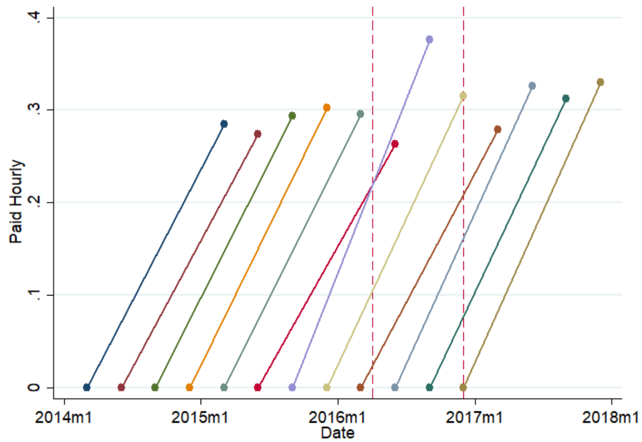
(a) Probability of Being Paid Hourly, by Date



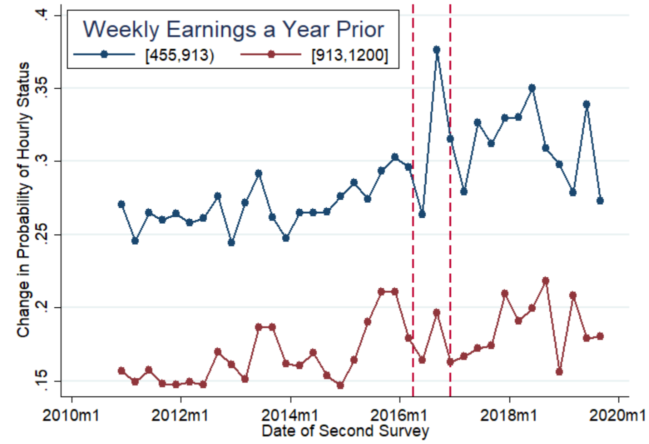
(b) Diff-in-diff for Hourly Status Indicator

Notes: Panel (a) shows the probability that an individual in the CPS is paid an hourly wage for each month between January 2010 and September 2019, conditional on weekly earnings. The two dotted vertical lines are at May 2016 and December 2016, respectively. Panel (b) shows the difference in difference estimates where I compare workers earnings earning between \$400 and \$1000 per week to workers earning between \$1000 and \$1200 per week.

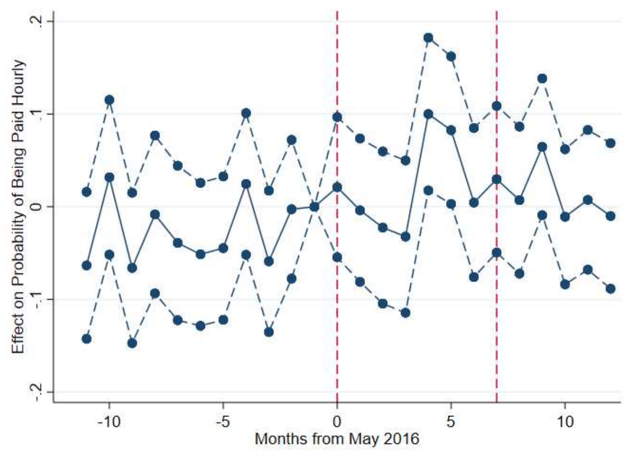
Appendix Figure I.4: Annual Change in Hourly/Salaried Status, Conditional on Initially Earning Between \$455 and \$913 per Week as a Salaried Worker



(a) Hourly Status, by Cohort



(b) Change in Hourly Status, by Earnings



(c) Difference-in-Difference of Hourly Status

Notes: In panel (a), the sample is restricted to workers who answered both outgoing rotation group surveys, and in their first CPS ORG survey, reported earning between \$455 and \$913 per week, and paid non-hourly. Each point represents the average response across all respondents in three consecutive surveys, starting with the month on the x-axis corresponding to that point. Each line connects the average response answered by the same panel of workers. In panel (b), the blue line is the difference between each pair of points in panel (a), plotted against the date of the second survey. The red line is the analogous graph for workers earning between \$913 and \$1200 in their first survey. Panel (c) plots the difference-in-difference estimates corresponding to the normalized difference between the two graphs in panel (b), computed using monthly data.